Third-Party Reporting, Deterrence Nudge, and Tax Compliance: Evidence from E-Commerce Firms in China

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Abstract

This study examines how a deterrent nudge, informed by third-party reporting, impacts tax compliance among e-commerce firms. A pop-up reminder presented by tax authorities in the Chaoyang district of Beijing to about 770 firms before electronic tax filing significantly boosted tax revenues. This result is driven entirely by the resolution of previously undisclosed tax obligations. The pop-up reminder also reduced the difference between sales volumes reported to tax authorities and actual e-commerce transaction volumes. This reduction is driven by a decrease in the latter, because firms stopped employing false transactions to game platform algorithms, rather than by an increase in sales volumes reported to tax authorities. Overall, the intervention proved most effective among larger firms. At a negligible cost, it generated about RMB 35 million in additional tax revenue and reduced sales misreporting by roughly RMB 2.7 million.

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1 Introduction

Tax compliance is the foundation of state capacity, as insufficient compliance undermines revenue generation, weakens fiscal institutions, and hampers the provision of public goods and services (Acemoglu, 2005; Besley and Persson, 2013). Information plays an essential role in tax governance (Pomeranz, 2015). Among different income categories, those with substantial information reporting are more likely to achieve higher levels of tax compliance compared to those with little or no information reporting.[1] Enforcement is substantially more effective when tax authorities can verify self-reported income against third-party information (Kleven et al., 2011; Pomeranz, 2015). However, while a body of literature has documented the effectiveness of third-party reporting for individual taxpayers, evidence regarding its impact on business, particularly in developing countries, remains scant (Carrillo et al., 2017; Agarwal et al., 2022). We provide new evidence on how third-party platform information can be used to detect underreporting and improve firm tax compliance in developing countries.

In recent years, the rapid development of the digital economy, particularly e-commerce, has introduced new challenges for tax enforcement. In 2024, e-commerce retail trade in the United States surpassed $1.17 trillion, constituting over 16% of total retail trade.[2] The e-commerce transactions are characterized by anonymity, high mobility, disintermediation, cross-regional operations, and paperless payments (Goolsbee, 2000; Basu, 2016), all of which contribute to a notably low level of tax compliance among e-commerce firms (Alm and Melnik, 2005; Einav et al., 2014; Argiles-Bosch et al., 2020). Current tax administration measures for e-commerce firms mainly depend on firm self-reporting, value-added tax (VAT) invoicing systems, and audits (Bibler et al., 2021).

Over the last two decades, China's adoption of a "deliberate inaction" strategy has been instrumental in enabling the swift expansion of its e-commerce industry (Liu, 2024). In 2023, China's e-commerce market reached $2.2 trillion,[3] driven by the proliferation of online platforms such as Alibaba, JD.com, and digital payment systems like Alipay and WeChat

[1] Source: Internal Revenue Service Research, Applied Analytics & Statistics Federal Tax Compliance Research: Tax Gap Estimates for Tax Years 2014-2016 Publication 1415 (Rev. 08-2022) Washington, DC.

[2] Data source: Retail Indicators Branch, U.S. Census Bureau, https://www.census.gov/econ/currentdata.

[3] Data source: The International Trade Administration, U.S. Department of Commerce. https://www.trade.gov/country-commercial-guides/china-ecommerce.

Pay.[4] Since 2020, China has initiated a comprehensive campaign to strengthen tax regulation in the digital economy, aiming to fix loopholes in tax collection and address the growing tax challenges. Specifically, China has devoted substantial efforts to incorporating third-party data from digital platforms into the tax administration system, enabling tax authorities to better detect potential tax fraudulent behaviors. In this context, we investigate how third-party reporting and behavioral interventions can be leveraged to improve tax compliance among e-commerce firms. In doing so, we propose a cost-effective strategy for the e-commerce industry (in China) that combines third-party information with deterrence nudges to enhance tax compliance.

We have collaborated with the Tax Bureau of Chaoyang District, Beijing, to carry out a policy experiment targeting local e-commerce firms. Using third-party data obtained from e-commerce platforms, the local tax authority estimated firms' actual sales revenue (hereafter referred to as *imputed sales*) and cross-validated these estimates against self-reported revenue, thereby identifying 6,198 firms suspected of underreporting. From this group, the local tax authority selected 773 firms to receive pop-up reminders via the electronic tax bureau (hereafter referred to as *E-Tax bureau*) system, requesting corrections to their tax filings and cautioning them about potential penalties for noncompliance. While traditional intervention methods, including in-person visits, letters, and phone calls, have been widely used in tax administration, pop-up notifications delivered through the E-Tax bureau provide a complementary communication channel. Because the E-Tax bureau is the exclusive official platform for corporate tax filing, these notifications can reach taxpayers precisely when they log in to prepare and submit their tax returns. Before accessing the declaration interface, e-commerce firms in the treated group were required to remain on the pop-up reminder page for 60 seconds. This design guarantees exposure to the intervention and effectively eliminates non-compliance with treatment arising from firms receiving but not viewing the message. Using confidential tax administrative data and third-party data provided by e-commerce platforms, we use event-study (ES) and difference-in-difference (DID) specification to evaluate the impact of third-party information and deterrent nudging on e-commerce tax

[4]In China, E-commerce platforms such as Taobao and JD.com are online marketplaces where retailers and consumers engage in online transactions with digital payment instruments, and platforms charge the retailers a certain percentage of their sales as a commission fee.

compliance.

Our analysis yields four main findings. First, underreporting of sales revenue is widespread among e-commerce firms, particularly among smaller firms. Second, a deterrent pop-up reminder based on third-party transaction information significantly improves tax compliance, reducing the difference between imputed and declared sales (hereafter referred to as *sales difference*) by about 44% and increasing tax payments. The resulting increase in tax payments primarily reflects the settlement of prior tax underpayments rather than an increase in contemporaneous tax liabilities. Third, the reduction in the sales difference is due to a decrease in the fraudulent sales activities that companies implement through e-commerce platforms to game the algorithms and increase their platform visibility. This phenomenon is called brushing and it refers to the deceptive practice of artificially boosting sales figures by employing individuals to pose as customers and implement fake transactions.[5] Fourth, the effects are stronger among larger firms, and the intervention generates approximately RMB 35 million in additional tax revenue at negligible implementation costs.

This paper makes three main contributions. We provide new evidence on third-party reporting and business tax compliance by showing how digital platform data can be transformed into a highly operational tax enforcement tool. Existing research proves that third-party reporting and VAT paper trails can substantially reduce tax evasion (Kleven et al., 2011; Pomeranz, 2015). Evidence from the United States further shows that third-party reporting through Form 1099-K improves small-business compliance, although the effects are incomplete and heterogeneous across firms (Slemrod et al., 2017; Adhikari, Alm, and Harris, 2021). However, the effectiveness of traditional third-party reporting methods can be limited, as firms may respond by overstating costs or shifting evasion to uncovered margins (Carrillo et al., 2017; Bjorneby et al., 2021). Rather than relying on standard third-party tax reports, we use transaction-level commission data from e-commerce platforms to infer sellers' actual sales revenues and identify underreporting. In doing so, we provide new evidence on the scale and distribution of tax non-compliance among business taxpayers in the

[5]The practice of "brushing" has been extensively documented in major media reports and is prohibited by e-commerce platforms and regulators. See https://www.wsj.com/articles/BL-CJB-26089, https://www.ft.com/content/735722e6-aca6-11e6-9cb3-bb8207902122, and the U.S. Postal Inspection Service warning at https://www.uspis.gov/news/scam-article.

digital economy. This is a rapidly growing sector in which direct observability is limited and enforcement is often costly (Goolsbee, 2000; Hoopes et al., 2016; Argiles-Bosch et al., 2020; Bibler et al., 2021).

We add to the growing literature on behavioral tax enforcement by studying a digitally delivered deterrence nudge targeted at firms suspected of underreporting on the basis of third-party information. This literature studies the impact of deterrence and non-deterrence nudges targeted at both businesses and individuals, as well as the efficacy of different delivery channels such as emails, phone calls, and SMS (see for example Antinyan and Asatryan, 2025 for a meta-study or Hallsworth et al., 2017; De Neve et al., 2021; Holz et al., 2023 for examples of country-specific contributions). Whether a timely deterrence nudge that appears as a pop-up reminder in the online filing system will have an impact on business taxpayers' compliance is an open yet important question to answer. It can help policymakers further streamline business taxpayers' reporting journeys, by introducing low-cost changes to the choice architecture that improve compliance.

We contribute to the emerging literature on heterogeneity in firm responses to tax enforcement (Brockmeyer et al., 2019; Holz et al., 2023; Gil et al., 2024). We show that the effects of the intervention are concentrated among larger firms, especially general taxpayers and non-micro and small enterprises (non-MSEs), both in reductions in sales differences and in increases in tax payments. This pattern suggests that the returns to information-based enforcement can be especially large among firms with bigger tax bases and more salient exposure to tax authority scrutiny. More broadly, our results highlight how digital technology can help tax authorities not only expand information capacity, but also target scarce enforcement resources toward the firms that generate the largest compliance gains.

The remainder of this paper is structured as follows. Section 2 illustrates the institutional background of the e-commerce law, the technological advancement of the tax administration in China, and the behavioral intervention procedure. Section 3 introduces the datasets and empirical methods we employed in our analysis. Section 4 details our empirical results. Section 5 concludes.

2 Background and Intervention Design

2.1 Institutional Background

With the continuous digitalization of China's tax administration, the normalized and institutionalized data exchange and coordination system between the tax authorities and other relevant departments such as banks, employers, and police has been gradually established. These institutional developments have considerably broadened the channels through which tax authorities can access third-party information from relevant administrative and commercial entities. At the same time, although e-commerce sector experienced rapid growth in China, the development of corresponding legislative regulations has been stagnant. To address the regulatory gap, China enacted the E-Commerce Law of the People's Republic of China (hereafter referred to as *the E-Commerce Law*) in 2018, which came into effect in 2019. The E-Commerce Law aims to safeguard the legitimate rights and interests of all parties involved in the e-commerce sector and to promote its orderly development.

The E-Commerce Law stipulates the obligation of e-commerce platforms (such as Alibaba and JD.com) to provide online transaction information to tax authorities.[6] Specifically, e-commerce platform businesses shall, by the laws and administrative regulations on tax collection administration, submit the identity information and the information related to tax payment of in-platform businesses to the tax authorities.[7] Since the E-Commerce Law came into effect, tax authorities have been legally empowered to obtain transaction data from e-commerce platforms and compare it with self-reported tax filings from merchants operating on those platforms. With the ongoing progress in the digital upgrading and intelligent transformation of tax collection and administration systems in China, tax authorities have started to employ big data and artificial intelligence technologies to implement automated regulatory mechanisms. By exploiting third-party data from e-commerce platforms, tax authorities can verify self-reported tax information against online transaction records,

[6] E-commerce platform operators serve as information intermediaries and have access to information about sales, payments, and transactions, etc.

[7] In addition, e-commerce platforms shall record and retain information on commodities and services, as well as transaction information released on the platform. The information on commodities, services, and transactions shall be retained for at least three years from the day the transaction is completed.

thereby improving the detection and deterrence of underreporting and tax evasion.

Enterprises in China are classified as general or small-scale taxpayers depending on whether their annual turnover exceeds RMB 5 million or not. General taxpayers are required to file tax returns and report their taxable income to the tax authorities on a monthly basis, while small-scale taxpayers complete these reporting obligations on a quarterly schedule. Accordingly, we collect both quarterly and annual tax payment data to capture the compliance responses of both groups. This taxpayer classification is based on tax administration rules and should be distinguished from the classification of firms by size. In addition to the taxpayer classification, China also has a separate classification of firms based on their size. In this paper, we distinguish between two definitions of MSEs. MSE(I) refers to the classification under the Corporate Income Tax Law of the People's Republic of China, which requires firms to operate in industries not restricted or prohibited by the state, with annual taxable income not exceeding RMB 3 million, fewer than 300 employees, and total assets not exceeding RMB 50 million. MSE(II) refers to the classification based on the National Bureau of Statistics (NBS) standards, which follow the Measures for the Classification of Large, Medium, Small and Micro Enterprises (2011) and its revised version issued in 2017.[8]

2.2 Behavioral Intervention

We focus on the compliance behavior of e-commerce companies registered in Chaoyang District, Beijing, operating on platforms such as Tmall and JD.com, the dominant B2C e-commerce platforms in China. According to public statistics released by the Local Municipal Bureau of Statistics, Chaoyang District is the most populous district in Beijing, with 3.442 million residents and an area of 470.8 square kilometers, roughly the size of the Principality of Andorra and equivalent to about 30% of the area of Greater London. The Chaoyang District is also the business center of Beijing, where multinational corporations, financial institutions, e-commerce platforms, and high-end service industries are located. Prior to the 2019 implementation of the E-Commerce Law, tax authorities lacked statutory access to third-party data from e-commerce platforms, which may have facilitated widespread misre-

[8]The detailed classification criteria for MSEs (II) are provided in the official guidelines available at: https://www.stats.gov.cn/sj/tjbz/gjtjbz/202302/t20230213_1902763.html.

porting of taxable income among online retailers.

In May 2020, to address the challenge brought by the digital economy, curb misreporting of e-commerce businesses, and reduce tax loss, tax administration in Chaoyang District, Beijing, conducted an innovative policy intervention that integrated third-party information with behavioral nudging. We obtained quarterly and annual administrative tax data for e-commerce firms in Chaoyang District from the local tax authority, covering tax years 2017 to 2020 and including both general and small-scale taxpayers. Tax authorities are able to examine potential tax evasion through the third-party transaction information provided by e-commerce platforms, including Tmall and JD.com. Specifically, to detect whether e-commerce firms underreport sales revenue, the local tax bureau constructs imputed sales revenue using third-party transaction information from e-commerce platforms. Since platforms charge merchants service commissions based on transaction values, the tax authority calculates imputed sales revenue by dividing the commission amount by the applicable commission rate. For example, if a platform charges a 5% commission and reports RMB 5,000 in commission fees, the corresponding imputed sales revenue is RMB 100,000. Subsequently, the local tax bureau compares the imputed sales revenue and actual sales amount declared by e-commerce companies. If the difference between the imputed sales and declared sales is positive, the corresponding firms are suspected of engaging in tax evasion.

7,585 e-commerce firms were registered in Chaoyang District, Beijing, and sold goods and services through Tmall and JD.com at the end of the 2019 tax year. Figure 1 demonstrates the grouping procedure. Based on big data analysis conducted by the local tax authorities using 2019 self-reported data and third-party information provided by e-commerce platforms, 6,198 firms were found to report sales revenue lower than their imputed sales revenue. This positive sales difference indicates a widespread tendency toward income underreporting and potential tax evasion. According to tax administrative data in Chaoyang District, Beijing, only 1,387 companies did not underreport their sales revenue, had a non-positive sales difference, and were classified into the *Normal* group. Among the 6,198 e-commerce companies that underreported their sales revenue, the local tax authority selected 773 firms to receive a reminder about the risk of underreporting sales revenue and the requirement to amend their returns; these firms constitute the *Suspected-intervened* group. The remaining e-commerce

firms exhibiting positive sales differences but not subjected to intervention were designated as the *Suspected-control* group. More specifically, the detailed grouping structure is provided below:

(a) *Normal* group: E-commerce firms whose imputed sales are not greater than the declared sales amount following big data inspections by tax authorities.

(b) *Suspected-control* group: Firms identified as engaging in tax evasion following a big data audit of third-party information provided by e-commerce platforms, where the difference between the estimated sales revenue and the self-reported sales revenue exceeds zero, but no reminder was sent to the enterprise.

(c) *Suspected-intervened* group: Firms identified as engaging in tax evasion after the big-data audit of the third-party information provided by the e-commerce platform where the difference between the estimated sales revenue and the self-reported sales revenue is greater than zero.

Unlike traditional correspondence methods such as letters, emails, and telephone calls, local tax administration employs a novel method to remind businesses of their obligations by adding a timely pop-up reminder through the E-Tax bureau to businesses (as shown in figure 2).[9,10] For each tax period, firms are obligated to submit tax declarations through the digital tax filing system administered by the Beijing E-Tax bureau within the statutory deadlines. When taxpayers log into the system to file their taxes, a reminder message is displayed in a pop-up window, which they are obliged to view for 60 seconds before selecting "Read" to proceed to subsequent filing procedures. The pop-up message contains a notification regarding risks identified through the cross-verification of imputed sales and self-reported sales figures. It serves as a reminder for firms to file tax returns truthfully, conduct self-assessments, amend previously submitted declarations where necessary, and settle any outstanding tax liabilities along with the corresponding statutory penalties.

[9]E-Tax Bureau is an online platform that provides tax authorities with a wide range of tax-related services over the internet. It is designed to streamline tax processes for both individuals and businesses, allowing them to handle tax filings, payments, invoicing, and other tax-related tasks online, reducing the need for in-person visits to tax offices. E-Tax bureau is a key component of China's Golden Tax Project IV and the main embodiment of using digital technology in taxation.

[10]Figure 2 demonstrates the detailed intervention content. Panel (a) presents the tax risk correction reminder, which precedes the detailed reminder content illustrated in Panel (b). Panels (c) and (d) provide English translations of Panels (a) and (b), respectively.

This intervention ensures that firms receive and acknowledge the notifications at negligible communication cost. This approach constitutes a cost-effective, convenient, and efficient means of disseminating information through digital systems.

According to the local tax authority, treated firms were selected from the suspected pool based on administrative and operational considerations. The selection was not random, but neither was it based on realized post-intervention compliance outcomes or the magnitude of firms' sales difference. In other words, treatment assignment occurred after firms had been flagged as potential underreporters using pre-intervention information, but before any post-intervention compliance responses could be observed. This feature is important for our empirical design: the treated and untreated firms were drawn from the same pool of ex ante suspected underreporters, which makes the untreated suspected firms a natural comparison group for evaluating the effect of the pop-up reminder.

3 Data and Methodology

3.1 Data

We combine confidential administrative datasets concerning tax payments and e-commerce transactions data provided by Tmall and JD.com and cover the period of 2017 to 2020 in Chaoyang District of Beijing, which comprise 25,619 observations on B2C e-commerce companies that participated in online retail activities. Table 2 presents the summary statistics of observable characteristics for the whole sample, *Normal* group, *Suspected-control* group, and *Suspected-intervened* group, respectively. The detailed description is provided in Online Appendix A.

We further establish four descriptive facts regarding e-commerce firms' tax compliance behavior. Unless otherwise specified, all statistics are based on pooled data from 2017 to 2020. Firstly, the underreporting of taxable revenue is prevalent in the e-commerce industry. In the period 2017 to 2020, 81.86% of e-commerce firms reported sales revenue that was less than the imputed sales revenue based on commission and commission rate, with an average sales difference of RMB 5.927 million. The widespread underreporting of taxable income reveals the severity of tax non-compliance within the e-commerce industry,

presenting a significant regulatory challenge for tax authorities in ensuring effective oversight of e-commerce sector.

Secondly, firm size is negatively associated with the probability of underreporting, with smaller e-commerce firms more likely to engage in underreporting behavior. Figure 3 presents the probability of firms underreporting at each percentile of operating income and sales revenue. According to panels (a) and (b), the less operating income or sales revenue a company has, the more likely it is to engage in underreporting behavior. Consequently, the extensive margin of tax compliance is lower for smaller businesses.

Thirdly, among companies that choose to underreport their sales revenue, the proportion of unreported sales revenue to imputed sales revenue is lower for those with higher operating income and sales revenue, but the proportion shows a growing trend in the top decile, as shown in panels (c) and (d). In other words, the intensive margin of tax compliance is also lower for smaller businesses, whereas the underreported proportion grows in the top decile.

Finally, micro and small enterprises (MSEs) and small-scale taxpayers underreport significantly more than non-MSEs and general taxpayers, both on extensive and intensive margins. Figure A1 illustrates the probability of underreporting and the proportion of underreported sales by firm type, showing that both the probability and the proportion of underreporting are significantly higher for MSEs and small-scale taxpayers.

According to these descriptive findings, underreporting in the e-commerce industry is widespread, with tax compliance being much lower among small businesses. This finding is consistent with tax gap estimates from the UK tax authorities (HMRC), which attribute around 60% of the UK tax gap to small businesses,[11] and with previous studies showing that tax compliance generally increases with firm size (Kochanova et al., 2016; Bachas et al., 2019).

3.2 Methodology

We employ difference-in-difference (DID) and event-study (ES) methods to evaluate the treatment effect of the intervention on e-commerce firms' compliance behavior. First, we use the *Suspected-Intervened* group as the treatment group. The control group consists of either

[11] Data source: https://www.gov.uk/government/statistics/measuring-tax-gaps/1-tax-gaps-summary.

the *Suspected-Control* group alone or the combined *Suspected-Control* and *Normal* groups, depending on the specification. We construct a DID model, as shown in equation (1), to estimate the changes in sales difference or overall tax burden between firms that received a pop-up notification and those that did not.

$$Y_{it} = \beta_0 + \beta_1 \text{Nudge}_i \times \text{Post}_t + \gamma X_{it} + Z_i f(t) + \mu_i + \delta_t + h_{jt} + \varepsilon_{it} \tag{1}$$

The dependent variable Y_{it} is firm-level outcome such as the firm i's sales difference or overall tax burden at year t,[12] Nudge_i refers to the indicator of whether the e-commerce firm received a pop-up reminder (=1) or not (=0). Post_t is a binary variable indicating the post-intervention period. Since the intervention was implemented in May 2020, Post_t takes a value of 1 if the tax authority has implemented the intervention (i.e., the year 2020) and a value of 0 if the intervention has not yet been implemented (i.e., the year 2017- 2019). X_{it} denotes a set of firm-level controls, including whether the firm is classified as an MSE(I) or MSE(II). Time-variant characteristics, such as sales revenue and operating income, may introduce potential confounding in the assignment of notification. To address this concern, we include interaction terms between average sales revenue or operating income in the pre-treatment years (i.e., 2017-2019) Z_i and a third-order polynomial function of time, $f(t)$, in equation (1). μ_i represents individual fixed effects used to control for the impact of firm-specific unobservable characteristics that do not change over time. δ_t denotes the year fixed effects to control for unobserved variables that may vary over time but are constant across firms. To flexibly absorb contemporaneous industry-level shocks that could otherwise confound the estimated treatment effects, we also control for the industry-by-year fixed effects h_{jt}, where j denotes the industry. ε_{it} denotes the error term. We use heteroskedasticity-robust standard errors clustered at the firm level. β_1 is the causal effect of the pop-up reminder on the changes in firms' sales differences and overall tax burdens.

To examine the validity of the pre-treatment parallel trend assumption underlying the design of equation (1), we estimate a dynamic difference-in-differences model, as specified

[12]The overall tax burden rate, obtained from tax administrative records provided by the tax authority, is defined as the ratio of a firm's total tax payments to its taxable operating revenue in the corresponding period.

in equation (2). This specification allows us to assess whether there are any significant differences in pre-treatment trends of sales difference or overall tax burden between the treatment and control groups.

$$Y_{it} = \beta_0 + \sum_{k=2017, k\neq 2019}^{2020} \beta_k \text{Nudge}_i \times 1_{it}(t = k) + \mu_i + \delta_t + h_{jt} + \varepsilon_{it} \tag{2}$$

In this specification, $1_{it}(t = k)$ is an indicator equal to one when $t = k$ and zero otherwise. The coefficients β_k capture the dynamic difference in the outcome variables (sales difference or overall tax burden) between the treatment and control groups in the year of 2017, 2018, 2020, with the year before the intervention (i.e., 2019) as the reference point. If the estimated coefficients β_k ($2017 \leq k < 2019$) are not significantly different from zero, the pre-treatment parallel trend assumption is satisfied.

Furthermore, we extend our analysis by conducting event-study estimations to examine the dynamic responses of firms' tax payments following the pop-up reminder intervention. Specifically, as shown in Equations (3) and (4), we estimate a parametric event-study specification and use the non-parametric event-study specification as a robustness check to examine the robustness of the dynamic treatment effects:

$$Z_{it} = \tau_0 + \tau \text{Post}_t + \gamma X_{it} + \mu_i + \varepsilon_{it} \tag{3}$$

$$Z_{it} = \tau_0 + \sum_{k=2017, k\neq 2019}^{2020} \tau_k 1_{it}(t = k) + \mu_i + \varepsilon_{it} \tag{4}$$

Where Z_{it} is tax paid by firm i at the year of t or its transformed version using inverse hyperbolic sine function. In the parametric event study equation (3), Post_t is the binary variable whether the intervention has been implemented, which takes the value of 1 if in the year of 2020, and takes the value of 0 in the years 2017-2019. The parameter τ represents the average treatment effect of pop-up reminder on corporate tax payment. If τ is estimated to be positive in the suspected-intervened group, the pop-up reminder has a positive impact on corporate tax payments. X_{it} denotes the firm-level controls, consistent with those in equation (1). The non-parametric event study analysis enables us to depict the dynamic

change of tax payment during our sampling period, and specify whether there is a distinct change after intervention. In the non-parametric event study setup as shown in equation (4), $1_{it}(t = k)$ is the indicator variable for the year $t = k$, where k ranges from 2017 to 2020 (excluding 2019); μ_i and ε_{it} are the same as those defined in equation (1). We choose the year before the event, the year of 2019, as the baseline period. If the estimated coefficients τ_k are not significantly different from zero before 2019, the parallel pre-trend assumption in this event study analysis is validated. To assess the long-term impact of the intervention on corporate tax payment dynamics, we also implemented an event study analysis based on equations (B1) and (B2) in appendix B using quarterly data.

4 Results

4.1 Baseline

4.1.1 Sales difference or overall tax burden

Figure 4 presents the time series of raw annual means of sales differences or overall tax burdens for treated and control firms in the main analysis sample. Panel (a) illustrates that pre-treatment sales differences for notified and control firms aligns for the period of 2017-2019, whereas a clear downward shift in sales differences for notified firms appears in 2020. Panel (b) shows that notified and control firms exhibit similar trends in overall tax burden during the pre-treatment years (2017–2019), while the overall tax burden of notified firms increases markedly in 2020. The pattern of sales difference or overall tax burden documents the effectiveness of the behavioral intervention on e-commerce firms'compliance behaviors.

Table 3 demonstrates the DID estimates of equation (1) for the effect of the intervention on treated firms' sales difference or overall tax burden. The results indicate that firms in the *Suspected–intervened* group exhibit significantly smaller sales differences and significantly higher overall tax burdens than firms in the control group, which consists of either the combined *Suspected-control* and *Normal* groups (panel (a)) or the *Suspected-control* group alone (panel (b)). Columns (1), (2), and (3) in panel (a) report the effect on sales difference under different model specifications: without controls, with controls, and with additional

controls for the time trend of sales revenue or operating income. According to column (3) in panel (a), compared to firms in *Suspected-control* and *Normal* groups, the intervention reduced the sales difference for the *Suspected–intervened* group by approximately RMB 2.737 million, representing a 44.174% reduction relative to the pre-treatment average of RMB 6.196 million. Columns (4), (5), and (6) in panel (a) present the estimated effects on the overall tax burden across different specifications. In comparison with the *Suspected-control* and *Normal* groups, the overall tax burden in the *Suspected-intervened* group increased by 2.6 percentage points, which is an over 100% increase compared to the average overall tax burden of 1.91% for the *Suspected-intervened* group during the period from 2017 to 2019. The results in panel (b) are directionally consistent with those in panel (a). The sales difference decreased by RMB 3.415 million, and the overall tax burden rose by 2.6 percentage points relative to the *Suspected-control* group. Empirical evidence from table 3 demonstrates that the intervention implemented by tax authorities significantly reduced the sales difference and increased the overall tax burden of e-commerce firms.

In Figure 5, we further corroborate our results by employing the dynamic panel specification outlined in equation (2). This specification helps to rule out the concern of differential pre-trends between the treated and controlled e-commerce firms. Panel (a) displays the dynamic effect on the sales difference. The estimated coefficients for the two years before the reminder intervention are not significantly different from zero. The dynamic effect on the overall tax burden is shown in panel (b). Similar to panel (a), the estimated coefficients before the intervention are also insignificantly different from zero. After the introduction of the intervention, an average statistically significant decrease of RMB 3.709 million in the sales difference and a statistically significant average increase of 2.436% in the overall tax burden were observed in 2020, which are in line with the estimates in Table 3.

4.1.2 Tax Payment

As a next step, we examine the impact of the intervention on e-commerce firms' tax payments. Table 4 reports the event study estimates of equations (3) and (B1) using the *Suspected-intervened* group as the analysis sample, with panel (a) demonstrating the effect on annual tax payment and panel (b) showing the effect on quarterly tax payment. Column

(1) and column (2) report the effect on annual tax payment with or without adding controls, respectively. Column (2) suggests that the annual tax payment increased on average by around RMB 0.058 million in 2020 compared to that in pre-treatment years. Columns (3) and (4) show the estimates with the inverse hyperbolic sine functional form of the tax payment as the outcome variable. The annual tax payment increased by 3.2% after our intervention. According to the estimates in column (5) and (6), the pop-up reminder increased the average quarterly tax payment by RMB 0.02 million. Based on the regression results with the inverse hyperbolic sine functional form, the quarterly tax payment increased by 1.6%. It is important to note that the increase in tax payments does not represent higher contemporaneous tax liabilities. Instead, it primarily reflects the settlement of previously underpaid tax liabilities identified through discrepancies between third-party transaction information and firms' reported sales. By directly affecting the 606 firms that continued operating in 2020, the intervention generated an estimated RMB 35.148 million in new tax revenue for the local government.[13] Accordingly, this behavioral nudge demonstrates a cost-effective approach to substantially increasing tax revenue yields with minimal administrative expense.

As shown in Figure 6, our results are further corroborated by the non-parametric event-study estimates from equations (4) and (B2). Panel (a) focuses on the effect on annual tax payments. Before the intervention, the coefficients on annual tax payments are not statistically significantly different from zero; however, we find a statistically significant average increase of RMB 0.053 million in 2020. In panel (b), the pre-intervention coefficients on quarterly tax payments are not statistically significantly different from zero, while a distinct increase of RMB 0.03 million in tax payments is observed in the second quarter of 2020, when the intervention occurred. Despite the fact that the deadline for amending tax returns is June 10, 2020, as stated in the pop-up reminder, the positive effect on tax payments has lasted into the fourth quarter of 2020.[14] Thus, our non-parametric analysis of quarterly tax payments reveals that the nudging intervention not only produces a significant immedi-

[13]The intervention initially covered 773 e-commerce firms. However, only 606 firms remained active in 2020 and were therefore included in the empirical analysis; the remaining firms had exited the market. The estimated RMB 35.148 million in additional tax revenue is obtained by multiplying the 606 firms by the estimated increase in tax payments per firm reported in column (2) of table 4.

[14]The filing period for the fourth quarter of 2020 was from January 1 to January 20, 2021, indicating that the effect persisted for more than six months after the amendment deadline.

ate effect when the reminder is sent, but also leads to a persistent effect on the long-term tax payment behavior. This long-term effect can be attributed to the deterrent mechanism of the nudge, which dissuades non-compliance by emphasizing the potential consequences associated with tax evasion.

4.2 Mechanism

Our baseline results indicate that the tax administration's pop-up reminder significantly reduced the sales difference, bringing the reported sales revenue closer to the imputed sales revenue post-intervention. This reduction effect on sales difference can be achieved through two potential channels. The first channel pertains to a decline in intentional underreporting. Following the receipt of the deterrent nudge, e-commerce firms significantly increase their reported sales revenue, indicating a behavioral response toward more truthful income declaration to tax authorities. The second channel involves a decline in artificial sales inflation. After firms receive the pop-up reminder, the imputed sales revenue calculated from commission invoice data obtained from e-commerce platforms significantly decreases, indicating that e-commerce firms reduce fraudulent activities such as "brushing".[15] Since store exposure is determined by sales performance via per-sale commission structures, online retailers may engage in brushing activities to artificially inflate their sales figures and gain greater visibility on the platform (Liu et al., 2023). Not only does brushing violate laws and regulations, but the income generated from such transactions also needs to be declared to tax authorities. Nevertheless, brushing is difficult to identify for supervising departments, despite being prevalent in the e-commerce industry.

To examine the underlying mechanism through which the nudging intervention takes effect, we separately estimate equation (1) with imputed sales, reported sales, and commission as outcome variables. The estimates are shown in Table 5. Columns (1), (2), and (3) show the effect on imputed sales under different specifications: without controls, with controls,

[15] Although no actual exchange of goods or services occurs, firms often incur platform commissions on these sham transactions in order to simulate legitimate sales activity within the platform's reporting system. Consequently, brushing behavior results in inflated imputed sales, as calculated from commission invoice collected from the platform. E-commerce firms may engage in order brushing to artificially inflate their reported sales figures. Higher reported sales can improve platform visibility and signal product popularity, potentially attracting more customers and increasing future revenues.

and with additional controls for the interaction between average pre-treatment sales revenue and operating income and a third-order polynomial function of time, *f(t)*. Imputed sales decreased by around RMB 2.550 million after the intervention relative to the control group (*Suspected-Control* and *Normal* groups). However, the estimates in columns (4)-(6), which capture the causal effect on reported sales, indicate that the nudging intervention did not lead to any significant change in reported sales. Columns (7)-(9) show a statistically significant decline of more than RMB 0.1 million in platform commission fees. Because firms incur commissions on artificial transactions, the observed decline is consistent with a reduction in costly sales-inflation practices such as brushing. By reminding firms that platform transaction data are visible to the tax authority, the intervention increases the perceived costs of inflating sales, making such practices less attractive. Combined with the absence of a significant increase in reported sales, the evidence suggests that the intervention primarily reduces artificial sales inflation rather than inducing higher contemporaneous income reporting. The results in columns (3), (6), and (9) show that even after including interactions between baseline firm characteristics and the polynomial function of time, the estimated effects remain robust and statistically significant, suggesting that these findings are not driven by time-varying confounders. Further evidence on the variation in imputed sales, reported sales, and commission fees over time is provided in appendix D. Taken together, the results in table 5 and appendix D suggest that, following the deterrent nudge, e-commerce firms did not increase their contemporaneous declared sales revenue. Instead, they reduced inflated transactions, such as brushing, leading to lower imputed sales and commission fees over time.

4.3 Robustness Check

To assess whether the baseline estimates are sensitive to sample composition, alternative comparison groups, placebo interventions, or violations of the identifying assumptions, Appendix C reports a set of complementary robustness checks. Overall, the robustness checks yield results consistent with our baseline findings: the deterrence pop-up reminder reduces the difference between imputed and reported sales and increases firms' tax payments.

First, appendix C1 re-estimates the main difference-in-differences models using two alternative samples: firms that remained active in 2020 and a balanced panel of firms observed

continuously from 2017 through 2020. Table C1 examines sales difference and overall tax burden, while table C2 examines annual and quarterly tax payments, including their inverse-hyperbolic-sine transformations. The estimated effects remain close to the baseline results in sign, magnitude, and statistical significance, indicating that the findings are not driven by firm exit or an unbalanced panel.

Second, appendix C2 tests for spillover effects and other contemporaneous shocks. We estimate annual and quarterly event-study models for firms in the *Normal* and *Suspected-control* groups, which did not receive the pop-up reminder. Figure C1 and table C3 show no significant post-intervention change in their tax payments. The absence of any change in tax payments among firms that did not receive the reminder suggests that no macroeconomic shocks broadly increased tax payments during the study period. The increase observed among notified firms is therefore more plausibly attributable to the deterrent nudge.

Third, appendix C3 conducts placebo tests. The pre-period placebo test reassigns the post-treatment period to 2019, when the intervention had not yet occurred, and finds no significant effect on sales difference, overall tax burden, or tax payments. We also implement a hybrid placebo procedure that randomly selects pseudo-treated firms and a pseudo-intervention date and repeats the estimation 500 times. As shown in Figure C2 and Table C4, the actual treatment effect lies in the tail of the placebo distribution, whereas the placebo estimates are centered around zero. These results make it unlikely that the baseline findings reflect random assignment or unobserved time-varying shocks.

Fourth, appendix C4 applies the HonestDID sensitivity analysis developed by Rambachan and Roth (2023) to account for possible violations of parallel trends. The analysis imposes restrictions on both the relative magnitude and the smoothness of post-treatment deviations from the pre-treatment trend. Figure C3 shows that the effect on sales difference remains robust when post-treatment deviations are allowed to be up to 1.1 times the pre-treatment deviations, and remains distinguishable from zero unless the deviation from linear extrapolation exceeds 3.2 percentage points. For overall tax burden, the corresponding relative-magnitude bound is 1.3, and the result remains robust unless the deviation from linear extrapolation exceeds 0.012 percentage points.

Fifth, appendix C5 examines whether the COVID-19 pandemic could confound the inter-

vention effect. Figure C4 shows no significant change in sales revenue or operating income for treated and control firms in 2020. Moreover, the intervention and comparison firms were located in the same district and were subject to the same local pandemic policies, making differential exposure to the common COVID-19 shock unlikely.

Finally, we also employ the coarsened exact matching (CEM) method to pair treated firms with comparable control firms from *Suspected-control* and *Normal* groups, with the estimation results reported in table C5. The CEM-DID estimates reported in appendix table C5 are consistent with the baseline results in tables 3 and 5.

4.4 Heterogeneity

There is no consensus in the literature regarding the heterogeneous effects of deterrent nudges on tax compliance by firm size. Some studies argue that smaller firms are more responsive to audit warnings because routine oversight is weaker (Pomeranz, 2015), whereas others suggest that they may be less responsive because perceived audit risk is low (Kleven et al., 2011). Kleven et al. (2016) further suggest that larger firms exhibit higher tax compliance because stronger internal controls constrain non-compliant behavior. Relatedly, Brockmeyer et al. (2019) show that the treatment improves filing compliance primarily among small firms, but strengthens payment compliance mainly among large firms, implying that the fiscal impact of the intervention is driven largely by large taxpayers. Consistent with this view, Holz et al. (2023) find stronger compliance responses to deterrence messages among larger firms. Gil et al. (2024) also document that information about the likelihood of future amnesties is most effective among large taxpayers. We contribute to this strand of the literature by clarifying how responses to deterrence-based information interventions vary by taxpayer size.

To examine the heterogeneity among firms' responses to the tax authority's interventions, we conduct a series of grouped regressions to determine how the intervention differed across different types of firms, which enables us to identify which firms are more sensitive to our deterrent nudging and explore the underlying mechanisms. Table 6 presents the estimated coefficients of equation (1) grouped by whether the firm is a MSE(I) or not and whether the firm is a general taxpayer or small-scale taxpayer. Columns (1) and (2) of panel (a) reveal that the sales difference declined by RMB 3.569 million for non-MSEs, representing

42.277% of the pre-intervention average. In comparison, the reduction for MSEs was smaller, amounting to RMB 0.597 million, 25.169% of the pre-intervention mean. Similarly, columns (3) and (4) show that general taxpayers experienced a decrease of RMB 3.659 million in sales difference following the intervention, equivalent to 56.492% of their pre-intervention average, while the effect for small-scale taxpayers is not statistically significant.

Panel (b) of Table 6 reports the effects of the pop-up reminder on firms' overall tax burden. The reminder increased the overall tax burden for both MSEs and non-MSEs, with a larger proportional effect among MSEs. Specifically, the overall tax burden increased by 4.8% for MSEs (141.2% relative to the pre-intervention mean), compared with 1.8% for non-MSEs (94.7% relative to the pre-intervention mean). This suggests that smaller firms exhibit a stronger proportional improvement in tax compliance. However, since MSEs have much lower baseline tax liabilities, these larger proportional gains translate into only modest increases in absolute tax payments. As shown in Appendix E, most of the additional tax revenue generated by the intervention comes from larger firms. These findings highlight that the fiscal impact of the intervention depends not only on firms' behavioral responses but also on their underlying tax liabilities. The heterogeneous mechanism analysis is also presented in appendix E.

To summarize, the effectiveness of the intervention is primarily driven by improved compliance among large enterprises. Firstly, the deterrent reminder significantly decreases sales difference of large enterprises, while its effect on smaller firms, such as MSEs and small-scale taxpayers, is relatively limited. This reduction in sales difference among large enterprises is primarily driven by a decline in brushing transactions, whereas the level of such activities among smaller firms remains largely unchanged following the intervention. Secondly, although both large and small enterprises increased their tax payments in response to the intervention, the overall rise in tax revenue is driven primarily by large enterprises. This finding echoes Brockmeyer et al. (2019), who show that the fiscal benefits of reminder interventions are concentrated among large taxpayers. The reason large e-commerce firms respond more to our deterrent intervention could be their higher non-compliance costs compared with smaller businesses. Large companies face greater financial burdens and more severe legal consequences when being detected in tax audits. Furthermore, large companies are more

likely to incur reputational and opportunity costs when engaging in tax non-compliance. As an example, public disclosure of fraudulent behaviors for large e-commerce firms can damage their brand reputation and cause the loss of customer trust. Furthermore, the reluctance of small firms to reduce brushing activities suggests their relatively weaker market competitiveness. Consequently, these firms may depend more heavily on fake orders to enhance visibility and compensate for their disadvantaged competitive position.

5 Conclusion

In the digital economy, the anonymity, high mobility, cashless payment methods, and high-frequency transactions that characterize e-commerce firms present substantial challenges for tax administration. Strengthening compliance while containing administrative costs is therefore of critical importance. Existing literature largely focuses on the lower compliance levels of e-commerce enterprises but offers limited guidance on cost-effective strategies to enhance tax collection and oversight. In this study, we propose an integrated approach that leverages third-party reporting by e-commerce platforms alongside deterrence-based nudges to improve tax compliance and mitigate fraudulent activities.

We combine platform transaction data with firms' self-reported sales revenue to identify potential underreporting among e-commerce firms in Chaoyang District, Beijing, and evaluate a deterrent pop-up reminder delivered through the electronic tax bureau. To the best of our knowledge, we provide the first evidence of combining third-party reporting and deterrent nudging to improve e-commerce tax compliance. Firms receiving a pop-up reminder via the E-Tax bureau system reduced the difference between imputed and reported sales by RMB 2.737 million. Their overall tax burden rose by 2.6% relative to the control group, with annual and quarterly tax payments increasing by 3.2% and 1.6%, respectively. The positive effect on tax payment persisted for three quarters even though the intervention ended in the second quarter of 2020. These increases primarily reflect the settlement of previously underpaid tax liabilities identified through discrepancies in reported sales, rather than contemporaneous increases in firms' tax liabilities.

Our findings shed light on the mechanism through which deterrent nudging takes effect.

The intervention works by curbing fraudulent transactions, thereby aligning the imputed sales revenue more closely with actual sales revenue. Beyond reminding firms to report and pay taxes accurately, the reminder message signals that they are subject to close monitoring, including of income from "brushing", thereby discouraging not only tax noncompliance but also related fraudulent activities.

Finally, the deterrent nudge is significantly more effective among larger firms. The intervention is considerably more effective among larger firms than among smaller firms, with stronger reductions in artificial sales inflation and larger increases in tax payments. In contrast, the effects are limited among MSEs and small-scale taxpayers. Larger enterprises demonstrate a stronger response to deterrent nudging, which can be partially attributed to the higher penalties and reputational risks associated with non-compliance behaviors. Additionally, larger firms tend to exhibit stronger market competitiveness compared to smaller firms, which are more likely to depend on brushing practices to increase their visibility and exposure in the marketplace. The implementation of nudging proves to be highly cost-effective. Our cost-effectiveness analysis proposes that the deterrent nudging yields RMB 35.148 million extra tax revenue and incurs negligible cost.

As countries around the world grapple with taxing the digital economy, China's approach offers critical insights into how strengthening platform accountability and advancing data interoperability can function as effective instruments for modernizing tax administration and ensuring the sustainability of public revenue in an increasingly digital and intangible economic environment.

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Figures and Tables

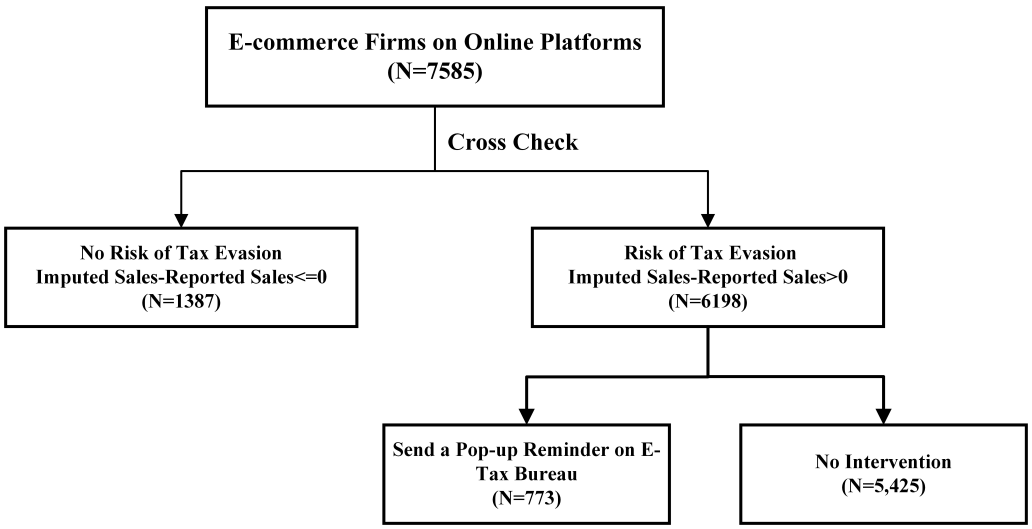


Figure 1: Grouping Procedure

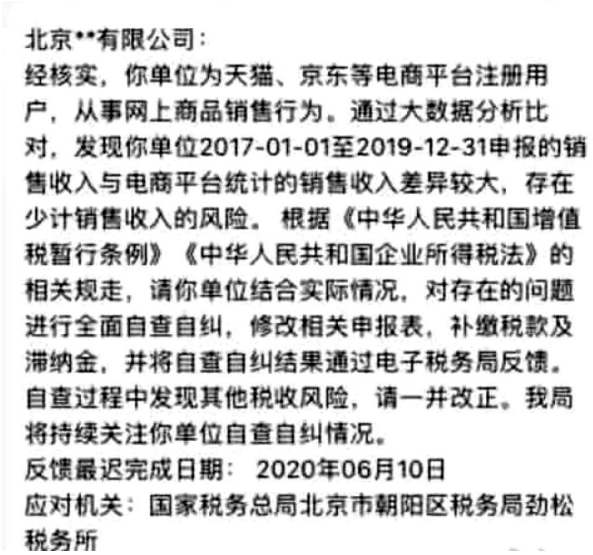
Notes: The grouping of e-commerce firms is shown in Table 1.

Table 1: Policy Intervention Groups

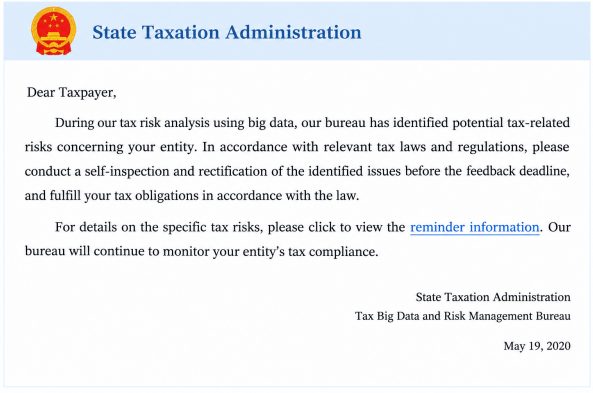
Group	Characteristic	Intervention	Sample Distribution
Normal group	According to the big data audit and cross-check conducted by the local tax administration, there is no positive difference between estimated and declared sales, indicating no tax evasion.	Do not intervene	1,387
Suspected-control group	After a big data audit and cross-check conducted by the local tax administration, there appears to be a positive difference between the estimated and declared sales, suggesting possible tax evasion. However, the intervention was not implemented.	Do not intervene	5,425
Suspected-intervened group	Based on big data audit and cross-check by the local tax administration, there appears to be a positive difference between the estimated sales and the declared sales, which indicates potential tax evasion. And intervention was implemented.	A pop-up reminder in the Electronic Tax Bureau system	773



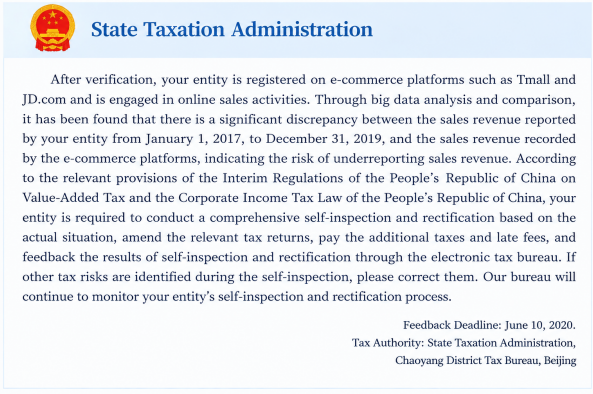
(a) Pop-up Reminder (Chinese)



(b) Reminder Content (Chinese)



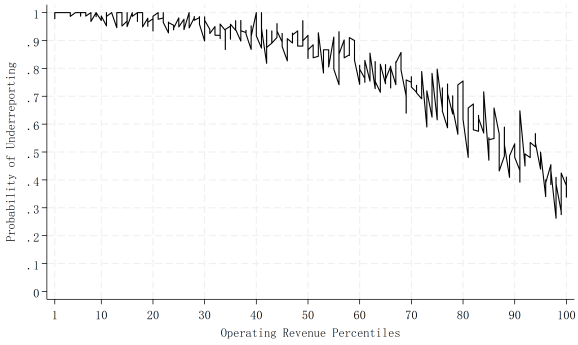
(c) Pop-up Reminder (English)



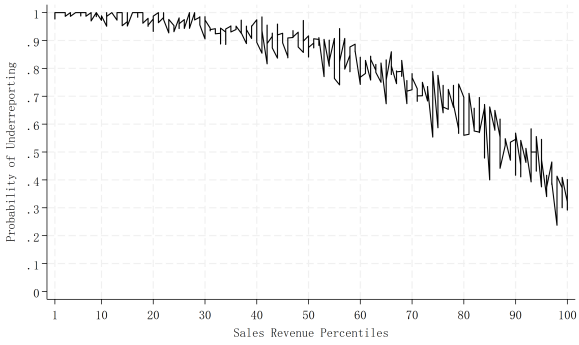
(d) Reminder Content (English)

Figure 2: Original Intervention Content

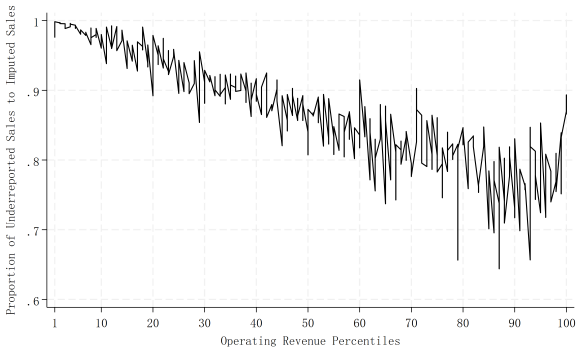
Notes: This figure presents the original content of targeted intervention messages sent by Chinese tax authorities to taxpayers. Panel (a) presents the tax risk correction reminder that precedes the detailed reminder content shown in panel (b). Panel (c) and panel (d) provide English translations of panel (a) and panel (b), respectively. These messages were delivered through the E-Tax bureau system, a platform that has played a pivotal role in modernizing tax administration in China. Since 2018, Chinese tax authorities have vigorously promoted the adoption of online tax services, allowing taxpayers to access the E-Tax bureau via computer to file tax returns and applications. This initiative has substantially enhanced the efficiency and convenience of tax compliance processes, reflecting the government's broader commitment to digital governance.



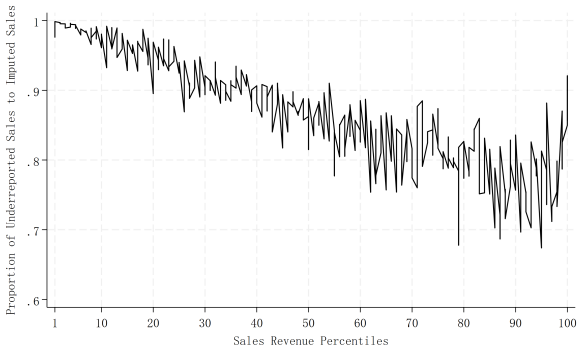
(a) Probability of underreporting by sales revenue percentiles



(b) Probability of underreporting by operating revenue percentiles



(c) Proportion of underreported sales to imputed sales by sales revenue percentiles

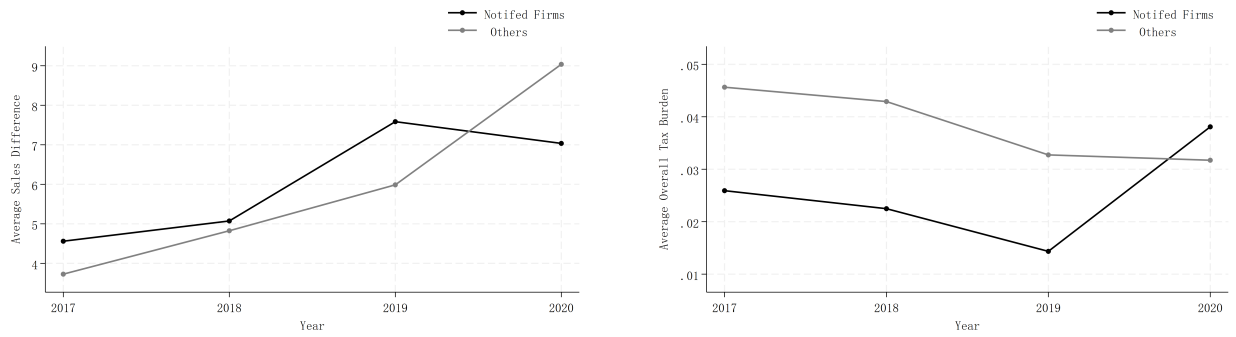


(d) Proportion of underreported sales to imputed sales by operating revenue percentiles

Figure 3: Extensive and Intensive Tax Compliance

Notes: This figure shows how extensive and intensive margins of tax compliance vary across different firm sizes. Panel (a) displays the underreporting probability distribution by sales revenue percentiles, and panel (b) reports the underreporting probability distribution by operating revenue percentiles. Panels (c) and (d) report the proportion of underreported sales to imputed sales by sales revenue percentiles and operating revenue percentiles, respectively.

Source: Authors' calculations.

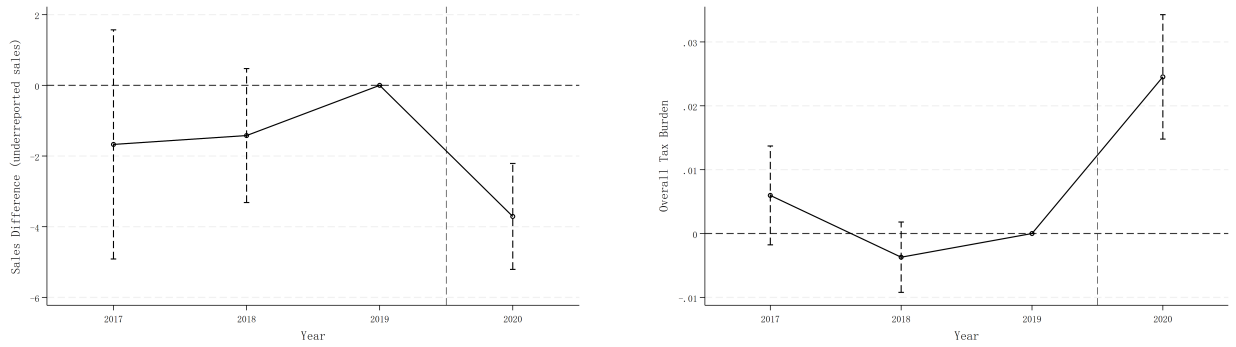


(a) Sales Difference (b) Overall Tax Burden

Figure 4: Dynamic Change of Sales Difference and Overall Tax Burden

Notes: The figure presents the time series of the raw annual means of sales differences and overall tax burdens for treated and controlled firms in the main analysis sample.

Source: Authors' calculations.

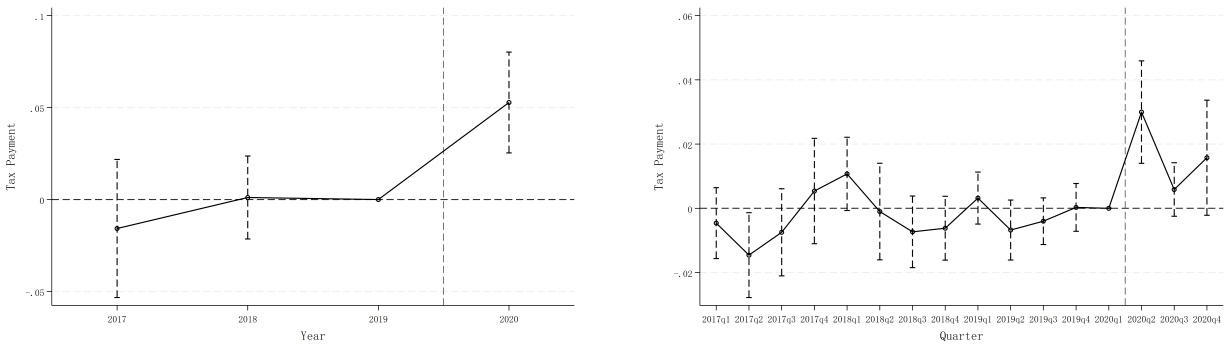


(a) Sales Difference (b) Overall Tax Burden

Figure 5: Effect on Sales Difference and Overall tax burden

Notes: Panel (a) demonstrates the effect of pop-up reminders on sales difference in comparison with *Suspected-control* and *Normal* groups, and panel (b) presents the effect of pop-up reminders on overall tax burdens relative to *Suspected-control* and *Normal* groups. The hollow markers represent the estimated effect of pop-up reminder (i.e., the coefficient β_k of the sampling years estimated from equation (2)). The dash lines are 95% confidence intervals of the estimated coefficients. The year before the intervention is omitted (i.e., the year 2019 is omitted).

Source: Authors' calculations.



(a) Annual Tax Payment

(b) Quarterly Tax Payment

Figure 6: Effect on Tax Payment

Notes: Panel (a) reports the non-parametric event study estimates of the effect of pop-up reminders on annual tax payment using the sample of the *Suspected-intervened* group (i.e., the estimates of τ_k from equation (4)). Panel (b) presents the non-parametric event study estimates of the effect of pop-up reminders on quarterly tax payments using the sample of the *Suspected-intervened* group (i.e., the estimates of ρ_q from equation (B2)). The hollow markers represent the estimated coefficients, and the dash lines are 95% confidence intervals of estimates. The year of 2019 is omitted in panel (a), and the first quarter of 2020 is omitted in panel (b).

Source: Authors' calculations.

Table 2: Descriptive Statistics

Variable	Whole Sample			Normal Group			Suspected-control Group			Suspected-intervened Group		
	N	Mean	SD	N	Mean	SD	N	Mean	SD	N	Mean	SD
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Operating revenue	25619	196.084	3620.802	4648	614.171	6,701.772	18758	114.852	2347.735	2213	6.510	19.441
Sales revenue	25619	158.021	2275.958	4648	441.136	2,308.719	18758	105.770	2365.371	2213	6.290	19.345
Reported sales	25619	2.244	28.004	4648	8.924	43.812	18758	0.849	23.115	2213	0.461	0.170
MSE(I)	25619	0.130	0.336	4648	0.005	0.072	18758	0.137	0.344	2213	0.332	0.471
MSE(II)	25619	0.775	0.418	4648	0.442	0.499	18758	0.835	0.371	2213	0.962	0.191
General taxpayer	25619	0.892	0.310	4648	0.984	0.136	18758	0.900	0.301	2213	0.635	0.482
Tax payment (annual)	25619	4.603	66.362	4648	17.216	113.717	18758	2.011	52.559	2213	0.086	0.424
Overall Tax Burden	25619	3.682%	0.079	4648	5.201%	0.059	18758	3.453%	0.084	2213	2.433%	0.067
Commission	25619	0. 405	3.806	4648	0.063	0. 371	18758	0.500	4.433	2213	0.320	0.734
Imputed sales	25619	8.171	80.258	4648	1.259	7.762	18758	10.085	93.490	2213	6.436	14.812
Sales difference	25619	5.927	76.501	4648	-7.665	42.173	18758	9.236	86.428	2213	6.426	14.862
Tax payment (quarterly)	105340	1.239	1.239	20699	4.383	33.124	76991	0.514	14.325	7650	0.026	0.142
Sales revenue (quarterly)	105340	42.277	586.447	20699	113.416	649.862	76991	27.173	596.042	7650	1.800	5.645

Notes: This table reports the number of observations, mean values, and standard deviations of the key variables for the whole sample and each subgroup. Sales revenue is firms' sales declared for VAT purposes, covering both online and offline activities. Reported sales denotes sales generated through e-commerce platforms that firms reported to the tax authority. Imputed sales refers to sales estimated by the tax authority by dividing platform commission payments by the applicable commission rate. Sales difference is imputed sales minus reported sales. All monetary variables are counted in millions. *Source*: Authors' calculations.

Table 3: Effect of Notification on Sales Difference and Overall Tax Burden

Panel (a): Whole Sample						
	Sales Difference			Overall Tax Burden		
	(1)	(2)	(3)	(4)	(5)	(6)
$\text{Nudge}_i \times \text{Post}_t$	-2.994***	-3.112***	-2.737***	0.025***	0.026***	0.026***
	(0.876)	(0.882)	(0.863)	(0.005)	(0.005)	(0.005)
Controls	No	Yes	Yes	No	Yes	Yes
$Z_i \times f(t)$	No	No	Yes	No	No	Yes
Firms FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry×Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Constant	5.998***	9.796***	13.431***	0.036***	0.029***	0.029***
	(0.021)	(1.155)	(2.856)	(0.000)	(0.002)	(0.002)
Observations	25,619	25,619	25,619	25,619	25,619	25,619
R^2	0.759	0.759	0.797	0.744	0.753	0.753
Mean in the pre-intervention years		6.196			0.019	
Panel (b): Suspected Sample						
	Sales Difference			Overall Tax Burden		
	(1)	(2)	(3)	(4)	(5)	(6)
$\text{Nudge}_i \times \text{Post}_t$	-3.535***	-3.688***	-3.415***	0.025***	0.026***	0.026***
	(1.122)	(1.131)	(1.085)	(0.005)	(0.005)	(0.005)
Controls	No	Yes	Yes	No	Yes	Yes
$Z_i \times f(t)$	No	No	Yes	No	No	Yes
Firms FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry×Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Constant	9.042***	14.675***	15.032***	0.033***	0.025***	0.025***
	(0.032)	(1.579)	(2.050)	(0.000)	(0.002)	(0.002)
Observations	20,971	20,971	20,971	20,971	20,971	20,971
R^2	0.766	0.766	0.814	0.761	0.770	0.770
Mean in the pre-intervention years		6.196			0.019	

Notes: This table reports the difference-in-difference estimates of equation (1). Panel (a) shows the results with the whole sample, panel (b) shows the results with e-commerce firms having positive sales difference (i.e., *Suspected-control* group and *Suspected-intervened* group. Nudge_i is the indicator variable for whether the enterprise received a pop-up reminder or not. Post_t is a binary variable whether is in the year of 2020 or not. Controls include indicators for whether the firm is classified as MSE (I) or MSE (II). We do not include the binary variable whether the firm is general taxpayer or small-scale taxpayer due to collinearity. Column (3) and Column (6) additionally control for interaction terms between average sales revenue or operating income in the pre-treatment years and a third-order polynomial function of time $f(t)$. Sales difference, operating income and sales revenue are counted in millions. All regressions control for firm fixed effects, year fixed effects, and industry-by-year fixed effects. Robust standard errors in parentheses are clustered at the firm level. *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Effect of Notification on Tax Payment

Variables	Panel (a): Annual Tax Payment				Panel (b): Quarterly Tax Payment			
	Tax Payment		Asinh(Tax Payment)		Tax Payment		Asinh(Tax Payment)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post_t / Post_s	0.055***	0.058***	0.030***	0.032***	0.020***	0.020***	0.015***	0.016***
	(0.017)	(0.018)	(0.008)	(0.008)	(0.006)	(0.006)	(0.004)	(0.004)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Firms FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	0.072***	0.087	0.060***	0.056*	0.022***	0.058**	0.021***	0.050**
	(0.005)	(0.059)	(0.002)	(0.030)	(0.001)	(0.026)	(0.001)	(0.022)
Observations	2,213	2,213	2,213	2,213	7,583	7,583	7,583	7,583
R^2	0.785	0.826	0.819	0.863	0.536	0.537	0.584	0.586
Mean in the pre-intervention years	0.071				0.022			

Notes: This table reports the event study estimates of the effect on annual and quarterly tax payment, and their inverse hyperbolic sine function form (asinh (tax payment)). Panel (a) shows the regression results of equation (3), and panel (b) shows the regression results of equation (B1). Post_t indicates the year 2020 in panel (a), while Post_s indicates quarters after the first quarter of 2020 in panel (b). All regression control for firm fixed effects. Robust standard errors in parentheses are clustered at the firm level. *** p<0.01, ** p<0.05, * p<0.1.

Table 5: Mechanism: Effect on Imputed Sales, Reported Sales and Commission

	Imputed sales			Reported sales			Commission		
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$\text{Nudge}_i \times \text{Post}_t$	-2.974***	-3.095***	-2.550***	0.020	0.016	0.187	-0.146***	-0.154**	-0.127***
	(0.891)	(0.898)	(0.864)	(0.095)	(0.096)	(0.147)	(0.043)	(0.054)	(0.042)
Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
$Z_i \times f(t)$	No	No	Yes	No	No	Yes	No	No	Yes
Firms FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	8.242***	12.204***	12.368***	2.244***	2.408***	-1.063	0.409***	0.592***	0.623***
	(0.021)	(1.187)	(1.808)	(0.002)	(0.129)	(2.610)	(0.001)	(0.040)	(0.088)
Mean in the pre-intervention years		6.241			0.045			0.309	
Observations	25,619	25,619	25,619	25,619	25,619	25,619	25,619	25,619	25,619
R^2	0.786	0.786	0.826	0.850	0.850	0.859	0.786	0.748	0.826

Notes: This table reports the difference-in-difference estimates of equation (1) with imputed sales, reported sales, and commission as outcome variables. The definitions of Nudge_i, Post_t are the same as in table 3. All regressions control for firm fixed effects, year fixed effects, industry-by-year fixed effects. We include interaction terms between average sales revenue and operating income in the pre-treatment years (i.e.,2017-2019) and a third-order polynomial function of time, $f(t)$. Robust standard errors in parentheses are clustered at the firm level. *** p<0.01, ** p<0.05, * p<0.1.

Table 6: Heterogeneous effect on sales difference and overall tax burden

Panel(a): Sales difference				
Variables	MSE(I) (1)	Non-MSE(I) (2)	General taxpayer (3)	Small-Scale taxpayer (4)
$\text{Nudge}_i \times \text{Post}_t$	-0.597***	-3.569***	-3.659***	-0.336
	(0.207)	(1.282)	(1.224)	(0.396)
Firms FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Industry×Year FE	Yes	Yes	Yes	Yes
Constant	1.167***	6.711***	6.477***	1.928***
	(0.011)	(0.025)	(0.021)	(0.030)
Observations	3,328	22,291	22,852	2,767
R-squared	0.939	0.760	0.759	0.806
Mean in the pre-intervention years	2.372	8.442	8.369	3.047
Panel (b): Overall tax burden				
Variables	MSE(I) (1)	Non-MSE(I) (2)	General taxpayer (3)	Small-Scale taxpayer (4)
$\text{Nudge}_i \times \text{Post}_t$	0.048***	0.018***	0.021***	0.033**
	(0.014)	(0.005)	(0.006)	(0.014)
Firms FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Industry×Year FE	Yes	Yes	Yes	Yes
Constant	0.066***	0.032***	0.036***	0.038***
	(0.001)	(0.000)	(0.000)	(0.001)
Observations	3,328	22,291	22,852	2,767
R^2	0.929	0.761	0.751	0.769
Mean in the pre-intervention years	0.034	0.019	0.022	0.028

Notes: This table reports the grouped difference-in-difference estimates of equation (1) by whether the firm is MSE(I) or not and whether the firm is a general taxpayer or small-scale taxpayer. The definitions of Nudge_i and Post_t are the same as in table 3. All regressions control for firm fixed effects, year fixed effects, industry-by-year fixed effects. Robust standard errors in parentheses are clustered at the firm level. *** p<0.01, ** p<0.05, * p<0.1.

Appendix A Summary Statistics Description

As reported in table 2, the average operating revenue in the whole sample is approximately RMB 196.084 million, sales revenue is about RMB 158.021 million, over 13% of all e-commerce firms are MSEs (I), around 77.5% of all e-commerce firms are MSEs (II), and 89.2% of all firms are general taxpayers, the total tax each firm paid is RMB 4.603 million on average, and the overall tax burden is 3.682% on average. The average commission e-commerce platform charged of each firm in the whole sample is RMB 0.405 million, the average imputed sales from the tax administration's big-data analysis are RMB 8.171 million

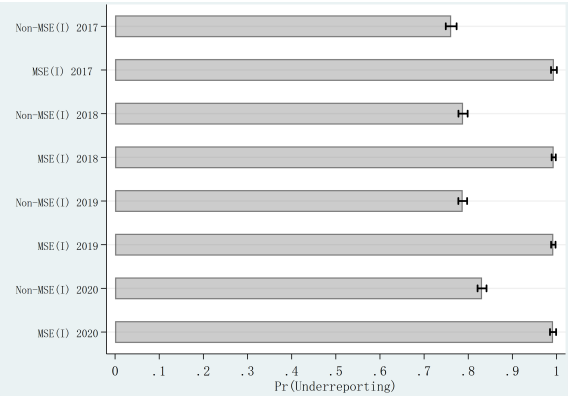
,[16] the average reported sales are RMB 2.244 million, and the sales difference in the whole sample of e-commerce enterprises was about RMB 5.927 million on average. The average quarterly tax payment is RMB 1.239 million and the average quarterly sales revenue is RMB 42.277 million.

In the *Normal* group, the average operating revenue reaches RMB 614.171 million, sales revenue averages RMB 441.136 million. The proportion of MSE (I) firms is 0.5%, while 44.2% fall into MSE (II). General taxpayers account for 98.4% of *Normal* group. The average platform commission is RMB 0.063 million. Imputed sales average RMB 1.259 million, whereas sales reported to the tax authority average RMB 8.924 million. The average difference between imputed and reported sales is negative, at RMB 7.665 million (i.e., −RMB 7.665 million). The average quarterly tax payment is RMB 4.383 million and the average quarterly sales revenue is RMB 113.416 million.

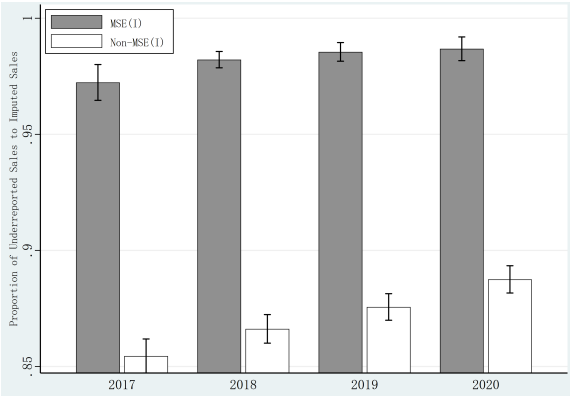
In the *Suspected-control* group, the average operating revenue is RMB 114.852 million, with average sales revenue of RMB 105.77 million. MSE (I) and MSE (II) account for 13.7% and 83.5%, respectively, while 90% are general taxpayers. Annual tax payments average RMB 2.011 million, with an overall tax burden of 3.453%. The average platform commission is higher at RMB 0.5 million, imputed sales average RMB 10.085 million, and the reported sales is RMB 0.849 million, the sales difference averages RMB 9.236 million. The average quarterly tax payment is RMB 0.514 million and the average quarterly sales revenue is RMB 27.173 million.

In the *Suspected-intervened* group, firms have an average operating revenue of RMB 6.51 million and sales revenue of RMB 6.29 million. Approximately 33.2% of businesses in this group fall under MSE (I), 96.2% are MSE (II), and 63.5% are general taxpayers. The annual tax payment averages RMB 0.086 million, with an overall tax burden of 2.433%. The average commission is RMB 0.32 million, imputed sales are RMB 6.436 million. The average reported sales revenue is as low as RMB 0.461 million. and the average sales difference is RMB 6.426 million. The average quarterly tax payment is RMB 0.026 million and the average quarterly sales revenue is RMB 1.8 million.

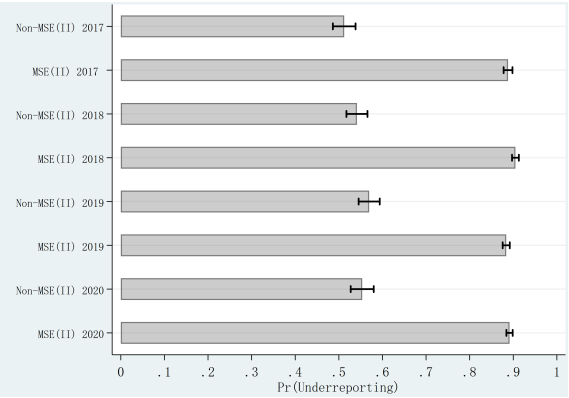
[16]The imputed sales refer to the estimated online sales revenue derived from the commission amount.



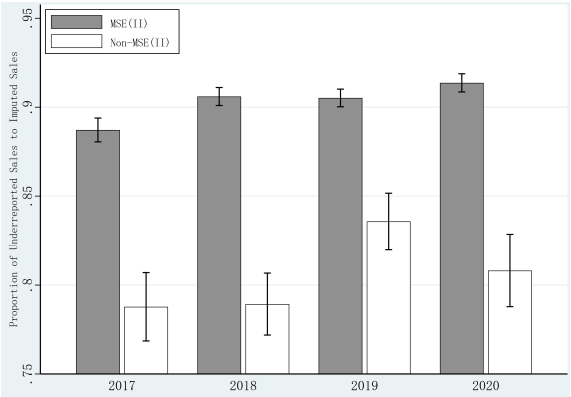
(a) Probability of Underreporting over MSE(I)



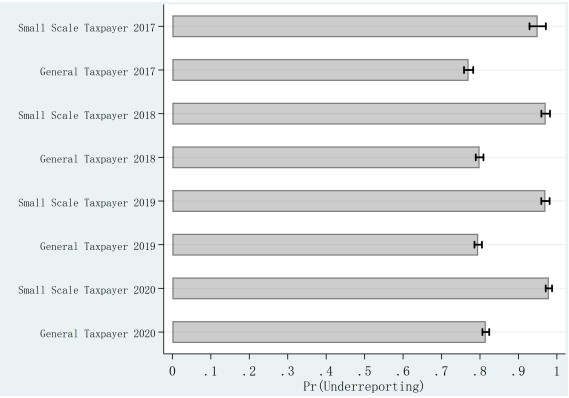
(b) Proportion of Underreported Sales to Imputed Sales over MSE(I)



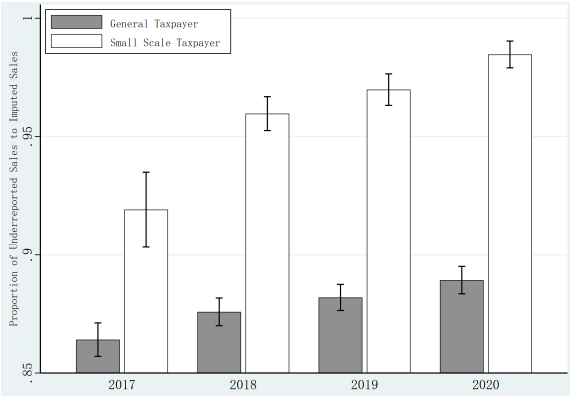
(c) Probability of Underreporting over MSE(II)



(d) Proportion of Underreported Sales to Imputed Sales over MSE(II)



(e) Probability of Underreporting over General/Small-Scale



(f) Proportion of Underreported Sales to Imputed Sales over General/Small-Scale

Figure A1: Extensive and Intensive Tax Compliance by Firm Type

Notes: This figure shows the extensive and intensive margin of tax compliance by firm type. Panel (a) displays the underreporting probability distribution by whether the firm is MSE(I) or not. Panel (b) reports the proportion of underreported sales to imputed sales by whether the firm is MSE(I) or not. Panel (c) displays the underreporting probability distribution by whether the firm is MSE(II) or not. Panel (d) reports the proportion of underreported sales to imputed sales by whether the firm is MSE(II) or not. Panel (e) displays the underreporting probability distribution based on whether the firm is a general taxpayer or a small-scale taxpayer. Panel (f) reports the proportion of underreported sales to imputed sales by whether the firm is a general taxpayer or a small-scale taxpayer.

Appendix B Quarterly Analysis

We identify quarterly patterns in corporate tax payments following the intervention and assess its long-term impacts, thereby improving our understanding of how the nudging intervention influences e-commerce tax compliance and whether it has a sustained positive effect on compliance behavior. Specifically, we estimate the following functions displayed in equation (B1) and (B2):

$$W_{is} = \rho_0 + \rho_1 \text{Post}_s + \gamma_1 \text{MSE}_{it}^{(\text{I})} + \gamma_2 \text{MSE}_{it}^{(\text{II})} + \mu_i + \varepsilon_{is} \tag{B1}$$

$$W_{is} = \rho_0 + \sum_{\substack{q=2017\text{Q1} \\ q \neq 2020\text{Q1}}}^{2020\text{Q4}} \rho_q 1_{is}(s = q) + \mu_i + \varepsilon_{is} \tag{B2}$$

W_{is} is tax paid by firm i at quarter s and its transformed version using inverse hyperbolic sine function. In the parametric event study model specified in equation (B1), Post_s is a binary indicator for the implementation of the intervention, taking the value of 1 for periods after the first quarter of 2020, and 0 otherwise. The parameter ρ_1 to be estimated from equation (B1) represents the average treatment effect of pop-up reminders on quarterly tax payments. If ρ_1 is estimated to be positive in the suspected-intervened group, the pop-up reminder has a positive impact on corporate tax payments. $\text{MSE}^{(\text{I})}$ and $\text{MSE}^{(\text{II})}$are indicator variables equal to one if firm i is classified as an MSE(I) or MSE(II), respectively, in year t, and zero otherwise. μ_i is firm fixed effect. In the non-parametric event study setup as shown in equation (B2), $1_{is}(s = q)$ is an indicator equal to one when $s = q$ and zero otherwise, where q ranges from the first quarter of 2017 to the fourth quarter of 2020 (excluding the first quarter of 2020); ε_{is} denotes the error term. We use heteroskedasticity-robust standard errors clustered at the firm level. We chose the quarter before the intervention, the first quarter in 2020, as the baseline period. If the estimated coefficients ρ_q are not significantly different from zero before the first quarter in 2020, the parallel pre-trend assumption in this event study analysis is validated. The non-parametric event study analysis enables us to depict the dynamic change of tax payment during our sampling period and specify whether

there is a persistent change after intervention.

Appendix C Robustness Check

C1 Resampling

To testify to the robustness of the estimates for the effect of pop-up reminders on sales difference and overall tax burden, we employed the sample that only included firms remain in business in 2020 and the balanced panel sample that persisted from 2017 to 2020 for estimation in table C1. Columns (1)-(3) in panel (a) present the DID estimates for sales difference using firms that remain in the sample in 2020, estimated sequentially without controls, with controls, and with additional controls for the time trends of sales revenue and operating income. The sales difference decreases by RMB 3.256 million and is statistically significant at the 1% level, which is in line with the estimates in table 3. Columns (4)-(6) in panel (a) show corresponding estimates using a balanced panel sample. The sales difference decreases by RMB 2.818 million and is statistically significant at the 10% level, similarly aligning with results in table 3. Columns (1)-(3) in panel (b) use firms that continued operations in 2020 as the analytical sample to demonstrate the impact of the intervention on the overall tax burden relative to the control group. The overall tax burden rose by 2.6 percentage points, which corresponds precisely with the estimations shown in table 3. As shown in columns (4)-(6) of panel (b), the overall tax burden increased by approximately 2.6 percent based on the balanced panel. This increase is in line with the estimation shown in table 3 as well.

As shown in table C2, we also use a sample of firms existing in 2020 and a balanced panel sample to test the robustness of the impact on tax payments. Tax payments on an annual basis are reported in columns (1)-(4), while tax payments on a quarterly basis are reported in columns (5)-(8). Columns (1) and (2) report annual tax payment estimates for the sample of firms active in 2020 and for the balanced panel, respectively. Columns (3) and (4) report the corresponding estimates using the inverse hyperbolic sine function of annual tax payment. Columns (5) and (6) show that quarterly tax payments increase after the intervention in both samples, while columns (7) and (8) report the corresponding estimates

for the inverse hyperbolic sine function of quarterly tax payment. These results are in line with the estimates in table 4.

C2 Spillover Effect

The deterrence nudging intervention took place in a jurisdiction with a population over 3 million, thereby reducing the likelihood of spillover effects. Nonetheless, for the purpose of examining the potential spillover effect as well as excluding influences from contemporaneous policy interventions, we conduct additional analyses. In figure C1, we plot the non-parametric event study estimates of equations (3) and (5) for the *Normal* and *Suspected-control* groups. According to panel (a) of figure C1, utilizing the year 2019 as the base year, we find that the tax payment in the *Normal* group and the *Suspected-control* group did not significantly change after the pop-up reminder was sent. Panel (b) of Figure C1 demonstrates that after the first quarter of 2020, there has been no significant change in the quarterly tax payments for the *Normal* group and the *Suspected-control* group. Table C3 reports the estimates from equations (3) and (5) for the *Normal* group and the *Suspected-control* group. Columns (1) and (2) report the impact on annual tax payments for the *Normal* and *Suspected-control* groups, while columns (3) and (4) present the corresponding estimates using the inverse hyperbolic sine function. Columns (5)-(8) report the impact on quarterly tax payments and their inverse hyperbolic sine form for the two groups. These estimates do not show significant increases after the nudging intervention.

The above results suggest that firms in the *Normal* group and the *Suspected-control* group did not change their tax payment behaviors significantly, indicating that the deterrent nudge intervention had no spillover effects. Moreover, even if there may be other underlying spillover effects, the difference-in-differences estimates shown in table 3 still support the conclusion that the intervention significantly increases taxpayer compliance. In such case, these estimates can be considered a lower bound of the proper treatment effect, which also suggests that the actual effect of the nudging intervention might be greater than our estimated values. To summarize, the potential spillover effects from the nudging intervention do not affect the validity of our findings.

C3 Placebo test

We also conduct a pre-period placebo test to validate the robustness of our research design. In particular, we reset our sample period to 2017-2019, during which time the nudging intervention was not implemented. We set the year 2018 as the base year and the year of 2019 as the post-treatment period. According to table C4, columns (1)-(2) present estimates of the effect on sales difference and overall tax burden, showing no significant change in 2019. In columns (3)-(4), we report the results of the parametric event study estimation on the tax payment for 2019, which also shows no significant change since 2018.

Following Chen and Yan (2023), We further implement a hybrid placebo test to ensure that the estimated treatment effects are not attributable to unobservable, time-varying confounding factors. In the original sample, 773 firms are selected as *Suspected-intervened* group. Accordingly, we randomly draw 773 firms from the full sample of 7,585 firms to construct a pseudo treatment group, with the remaining firms serving as the pseudo-control group. Additionally, one pseudo-treatment time is randomly chosen from the range of observed periods to simulate the timing of an intervention. We subsequently conduct DID estimations for both sales difference and overall tax burden, repeating the procedure 500 times to construct the distribution of placebo effects. As illustrated in panel (a) of figure C2, the estimated treatment effect on sales difference (represented by the vertical solid line) lies in the left tail of the placebo effect distribution. Given that the placebo estimates for sales differences are centered around zero, this provides compelling evidence that the observed treatment effect on sales difference shown in table 3 is unlikely to result from random variation or unobserved time-varying confounders. Similarly, as shown in panel (b) of figure C2, the estimated treatment effect (represented by the vertical solid line) on overall tax burden falls in the right tail of the placebo effect distribution. And the placebo estimates for overall tax burdens are also centered around zero, suggesting that the estimated effect on overall tax burden shown in table 3 is not attributable to random variations or unobserved time-varying shocks.

C4 Accounting for Potential Violation of Parallel Trends

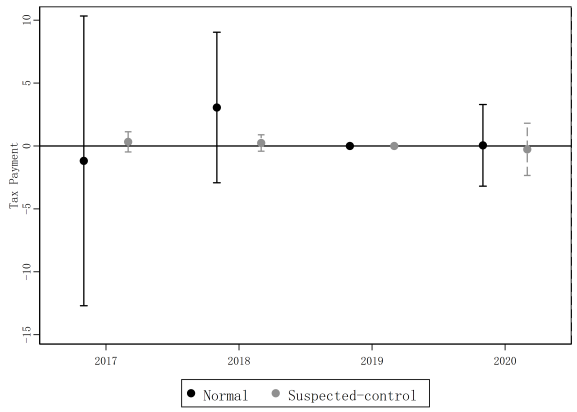
Despite the intuitive nature of pre-trends tests, recent studies indicate that they may exhibit low statistical power (Freyaldenhoven et al., 2019; Kahn-Lang and Lang, 2020; Roth, 2022), and that conditioning the analysis on the successful completion of pre-trends tests introduces statistical complications associated with pre-testing. As Roth (2022) pointed out, conventional statistical tests applied to pre-treatment periods often lack sufficient power to detect meaningful deviations, thereby limiting their effectiveness in validating the parallel trends assumption underpinning DID designs. To mitigate this issue, we implement the Honest-DID methodology developed by Rambachan and Roth (2023), which allows for sensitivity analysis under plausible deviations from the pre-treatment parallel trends assumption. To evaluate the sensitivity of our results to potential violations of the parallel trends assumption, we implement robustness checks that impose restrictions on both the relative magnitudes and smoothness of the deviations of parallel trends in the post-treatment period. These assumptions allow us to bound the degree to which pre-treatment trends may deviate from parallelism, thereby enabling robust inference under plausible violations of the identifying assumption.

Panels (a) and (b) of figure C3 present the sensitivity analyses for the DID specifications outlined in equation (2), using *sales difference* as the outcome variable, the post-treatment violation of parallel trends is no more than 1.1 times that in the pre-treatment period, and we can reject a null effect unless we are willing to allow for the linear extrapolation across consecutive periods to be off by more than 3.2 percentage points. Similarly, Panels (c) and (d) of figure C3 show the sensitivity analyses for the same DID specifications, using *overall tax burden* as the outcome variable. The estimated effects remain robust under a relative magnitude restriction allowing post-treatment deviations up to 1.3 times the magnitude of pre-treatment deviations, and the estimated effect is robust unless deviations from linear trend extrapolation exceed 0.012 percentage points across consecutive periods.

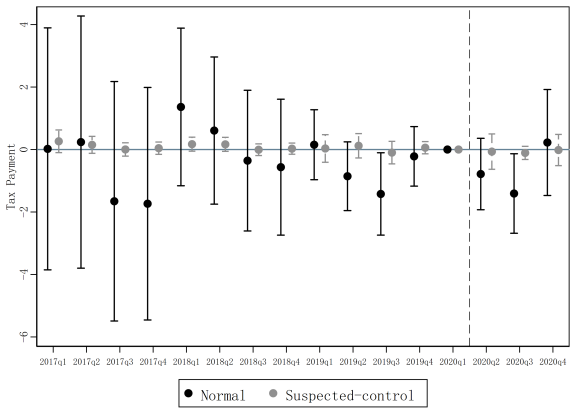
C5 Covid-19

A potential confounding factor in this study is the COVID-19 pandemic. Existing research suggests that the pandemic led to a surge in online purchasing(Cavallo et al., 2022; Alexander and Karger, 2023). However, other studies indicate that China's stringent lockdown policies may have negatively impacted e-commerce sales (Han et al., 2022). Notably, the policy intervention examined in this study was implemented in May 2020, during the height of the pandemic. Therefore, it is important to acknowledge that the pandemic shock may have simultaneously influenced e-commerce dynamics, thereby potentially confounding the estimated effects of the policy intervention.

Nevertheless, as shown in figure C4, the sales revenue and operating income of e-commerce firms did not exhibit significant change in 2020, suggesting that e-commerce sector as a whole was relatively stable during this period. Furthermore, China implemented nationwide lockdown during the first and second quarters of 2020, affecting all regions across the country. Our intervention was conducted within the same county in Beijing, where government imposed COVID-19 policies, such as lockdowns, were uniformly implemented at the county level or higher. Consequently, any potential impact of COVID-19 on e-commerce firms would have affected both the treatment and control groups equally. This ensures that the observed effects are not confounded by heterogeneous impacts of the pandemic, thereby mitigating its potential bias in our estimations.



(a) Annual tax payment of *Normal* and *Suspected-control* firms

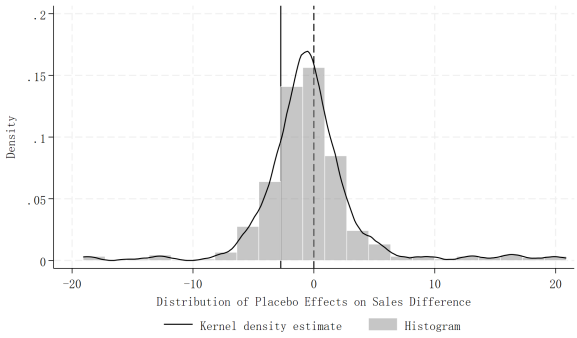


(b) Quarterly tax payment of *Normal* and *Suspected-control* firms

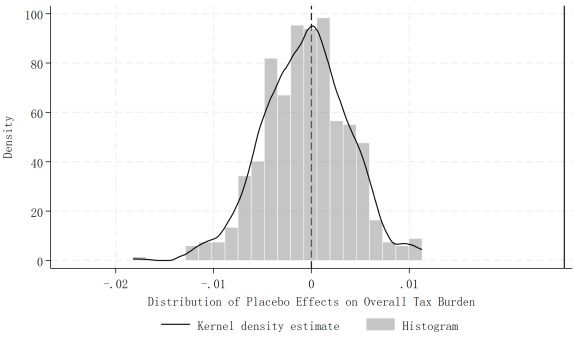
Figure C1: Robustness Check on Annual and Quarterly Tax Payment

Notes: Panel (a) reports the non-parametric event study estimates of τ_k from equation (4) for the Normal group and the Suspected Control group. Panel (b) presents the non-parametric event study estimates of the effect of pop-up reminders on quarterly tax payments for the Normal group and the Suspected Control group (i.e., the estimates of ρ_q from equation (B2)). The hollow markers represent the estimated coefficients, and the dash lines are 95% confidence intervals of estimates. The year of 2019 is omitted in panel (a), and the first quarter of 2020 is omitted in panel (b).

Source: Authors' calculations.



(a) Hybrid Placebo Test: Sales Difference

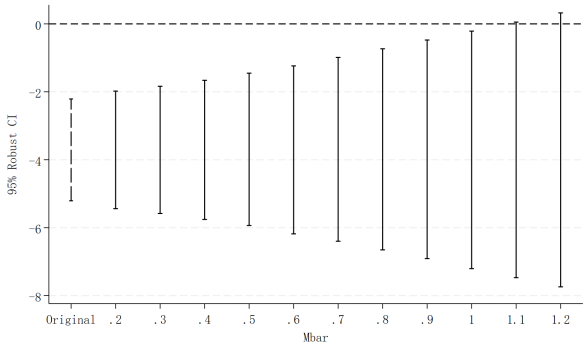


(b) Hybrid Placebo Test: Overall Tax Burden

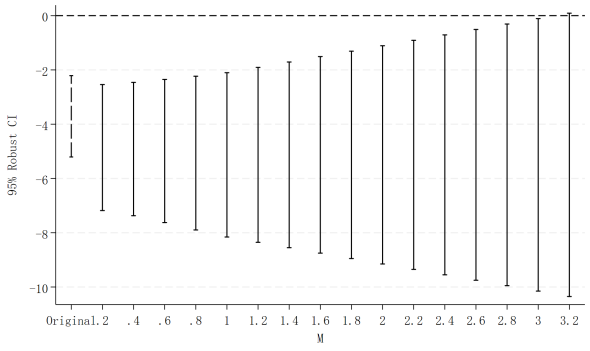
Figure C2: Hybrid Placebo Test

Notes: Panel (a) reports the distribution of hybrid placebo effects on sales difference; the black solid line represents the estimated treatment effect on sales difference. Panel (b) presents the distribution of hybrid placebo effects on overall tax burden; the black solid line represents the estimated treatment effect on overall tax burden.

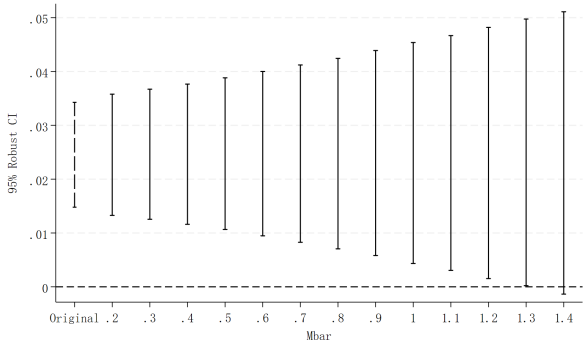
Source: Authors' calculations.



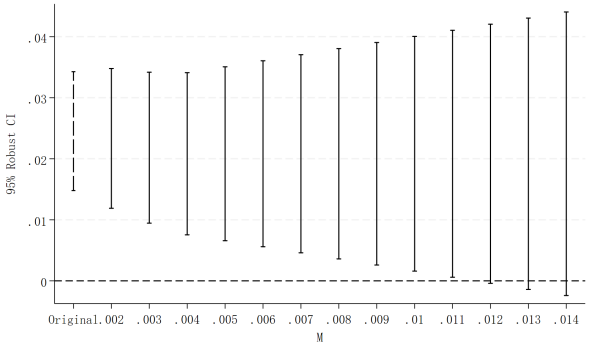
(a) Relative Magnitude Restrictions: Sales Difference



(b) Smoothness Restrictions: Sales Difference



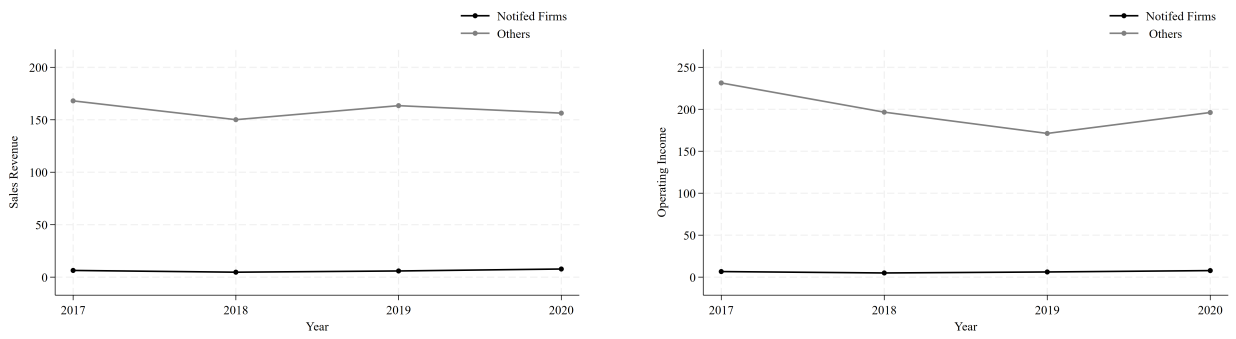
(c) Relative Magnitude Restrictions: Overall Tax Burden



(d) Smoothness Restrictions: Overall Tax Burden

Figure C3: Sensitivity Analysis for Potential Violations of Parallel Trends

Notes: This figure shows the sensitivity analysis for the DID specifications in equation (2), representing 95% robust confidence intervals constructed under plausible restrictions on violations of the parallel trend assumption. Panels (a) and (b) present the relative magnitude and smoothness restriction testing results for the treatment effect on sales difference. Panels (c) and (d) demonstrate the relative magnitude and smoothness restriction testing results for the treatment effect on overall tax burden.



(a) Change in Sales Revenue

(b) Change in Operating Income

Figure C4: Changes in Sales Revenue and Operating Income

Notes: Panel (a) illustrates the evolution of average sales revenue from 2017 to 2020 for both treated and firms in the control group. Panel (b) depicts the changes in average operating income between 2017 and 2020 for treated firms and firms in the control group.

Source: Authors' calculations.

Table C1: Robustness Checks on Sales Difference and Overall Tax Burden

Panel (a): Sales difference						
	(1)	(2)	(3)	(4)	(5)	(6)
$\text{Nudge}_i \times \text{Post}_t$	-3.336***	-3.518***	-3.256***	-3.332*	-3.656**	-2.818*
	(1.052)	(1.062)	(1.023)	(1.772)	(1.769)	(1.695)
Controls	No	Yes	Yes	No	Yes	Yes
$Z_i \times f(t)$	No	No	Yes	No	No	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry×Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Constant	11.305***	18.281***	18.245***	17.617***	25.323***	22.596***
	(0.043)	(1.986)	(2.595)	(0.825)	(2.281)	(3.087)
Observations	14,727	14,727	14,727	7,191	7,191	7,191
R^2	0.791	0.791	0.850	0.795	0.795	0.860
Panel (b): Overall tax burden						
	(1)	(2)	(3)	(4)	(5)	(6)
$\text{Nudge}_i \times \text{Post}_t$	0.025***	0.026***	0.026***	0.024**	0.026**	0.026**
	(0.005)	(0.005)	(0.005)	(0.009)	(0.010)	(0.010)
Controls	No	Yes	Yes	No	Yes	Yes
$Z_i \times f(t)$	No	No	Yes	No	No	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry×Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Constant	0.027***	0.023***	0.023***	0.029***	0.026***	0.026***
	(0.000)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)
Observations	14,727	14,727	14,727	7,191	7,191	7,191
R^2	0.612	0.623	0.623	0.507	0.593	0.593

Notes: This table reports the DID estimates based on two samples: (i) firms that were active in 2020, and (ii) a balanced panel comprising firms that continuously operated from the first quarter of 2017 through the fourth quarter of 2020. Panel (a) presents the effects on sales differences and panel (b) reports the effects on overall tax burden. Columns (1)-(3) use the panel of firms still active in 2020, reporting results without controls, with control and with additional control for time trends of sales revenue and operating income, robust standard errors clustered at the firm level are reported in parentheses. Columns (4)-(6) provide corresponding estimates based on the balanced panel, presenting results without controls, with control and with additional control for time trends of sales revenue and operating income, heteroskedasticity-robust standard errors are reported in parentheses. The definitions of Nudge_i, Post_t and Controls are the same as those in Table 3. All regressions control for firm fixed effects, year fixed effects, and industry-by-year fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Table C2: Robustness Check on Tax Payment of Suspected-intervened Group

Variables	Panel (a): Annual Tax Payment				Panel (b): Quarterly Tax Payment			
	Tax Payment		Asinh(Tax Payment)		Tax Payment		Asinh(Tax Payment)	
	Active	Balanced	Active	Balanced	Active	Balanced	Active	Balanced
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\text{Post}_t/\text{Post}_s$	0.055***	0.076*	0.030***	0.038*	0.020***	0.026**	0.015***	0.018**
	(0.017)	(0.039)	(0.008)	(0.020)	(0.006)	(0.012)	(0.004)	(0.008)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	0.082***	0.114***	0.067***	0.096***	0.023***	0.030***	0.022***	0.029***
	(0.006)	(0.010)	(0.003)	(0.005)	(0.001)	(0.002)	(0.001)	(0.001)
Observations	1,786	660	1,786	660	6,740	2,739	6,740	2,739
R^2	0.784	0.709	0.819	0.755	0.536	0.408	0.585	0.468

Notes: This table presents the results of an event study estimation based on two samples: (i) a panel including firms that were still active in 2020, and (ii) a balanced panel comprising firms that continuously operated from the first quarter of 2017 through the fourth quarter of 2020. Columns (1)-(4) display annual estimates, and columns (5)-(8) display quarterly estimates. Columns (1), (2), (5), and (6) use tax payment as the outcome variable, while columns (3), (4), (7), and (8) use its inverse hyperbolic sine version. Within each outcome pair, the first column uses the sample of firms active in 2020, and the second column uses the balanced panel sample. The post-intervention indicators are Post_t for annual outcomes and Post_s for quarterly outcomes, as defined in equations (3) and (B1), respectively. All regressions control for firm-level fixed effects. Robust standard errors in parentheses are clustered at the firm level. *** p<0.01, ** p<0.05, * p<0.1.

Table C3: Robustness Check on the Tax Payment of Normal Group and Suspected-control Group

Variables	Panel (a): Annual Tax Payment				Panel (b): Quarterly Tax Payment			
	Tax Payment		Asinh(Tax Payment)		Tax Payment		Asinh(Tax Payment)	
	Normal	Suspected-control	Normal	Suspected-control	Normal	Suspected-control	Normal	Suspected-control
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\text{Post}_t/\text{Post}_s$	-0.661	-0.403	-0.080***	-0.036***	-0.655	-0.064	-0.060***	-0.018***
	(2.837)	(0.918)	(0.023)	(0.009)	(0.590)	(0.204)	(0.013)	(0.004)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	17.343***	2.105***	1.713***	0.372***	4.519***	0.533***	0.839***	0.153***
	(0.542)	(0.214)	(0.004)	(0.002)	(0.119)	(0.046)	(0.003)	(0.001)
Observations	4,648	18,758	4,648	18,758	20,681	76,339	20,681	76,339
R^2	0.878	0.817	0.965	0.886	0.721	0.673	0.844	0.774

Notes: This table reports the event study estimation of the nudging intervention on annual and quarterly tax payment for the *Normal* group and *Suspected-control* group (i.e., the estimated coefficients from equations (4) and (6). Columns (1)-(4) report annual estimates, and columns (5)-(8) report quarterly estimates. Columns (1), (2), (5), and (6) use tax payment as the outcome variable, while columns (3), (4), (7), and (8) use its inverse hyperbolic sine version. Within each outcome pair, the first column reports estimates for the *Normal* group, and the second column reports estimates for the *Suspected-control* group. The post-intervention indicators are Post_t for annual outcomes and Post_s for quarterly outcomes, as defined in equations (3) and (B1), respectively. All regressions control for firm-level fixed effects. Robust standard errors in parentheses are clustered at the firm level. *** p<0.01, ** p<0.05, * p<0.1.

Table C4: Placebo Test

	Sales difference	Overall Tax Burden	Tax payment	Asinh(tax payment)
	(1)	(2)	(3)	(4)
$\text{Nudge}_i \times \text{Post}_t$	1.368	0.001		
	(1.108)	(0.002)		
Post_t			0.004	-0.003
			(0.010)	(0.005)
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes		
Industry×Year FE	Yes	Yes		
Constant	5.012***	0.038***	0.069***	0.061***
	(0.043)	(0.000)	(0.005)	(0.002)
Observations	19,754	19,754	1,607	1,607
R^2	0.741	0.827	0.853	0.886

Notes: This report reports the difference-in-difference estimates of equation (1) and parametric event study estimates of equation (4). The definition of $\text{Nudge}_i$is the same as Table 3. Post_t is a binary variable that determines whether it is the year after 2018 or not. The regressions in columns (1) and (2) control for firm fixed effects, year fixed effects and industry-by-year fixed effects. The regressions in columns (3) and (4) control for firm fixed effects. Robust standard errors in parentheses are clustered at the firm level. *** p<0.01, ** p<0.05, * p<0.1

Table C5: Main Results After CEM Matching

	Sales Difference	Overall Tax Burden	Imputed sales	Reported sales	Commission
	(1)	(2)	(3)	(4)	(5)
$\text{Nudge}_i \times \text{Post}_t$	-1.785***	0.028***	-1.806***	-0.022	-0.092***
	(0.632)	(0.005)	(0.644)	(0.030)	(0.032)
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Industry×Year FE	Yes	Yes	Yes	Yes	Yes
Constant	3.827***	0.040***	4.203***	0.376***	0.213***
	(0.015)	(0.000)	(0.016)	(0.001)	(0.001)
Observations	25,347	25,347	25,347	25,347	25,347
R^2	0.708	0.782	0.712	0.901	0.715

Notes: This table presents the main results derived from CEM-DID analysis. We applied Coarsened Exact Matching (CEM) to improve comparability between treated and control firms. Continuous covariates sales revenue and operating income were coarsened into 100 bins each, while binary indicators MSE(I) and MSE(II) were included without coarsening. Firms that could not be exactly matched on the coarsened covariates were excluded from the analysis. Standard errors are clustered at the firm level. The regression specifications include firm fixed effects, year fixed effects and industry-by-year fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

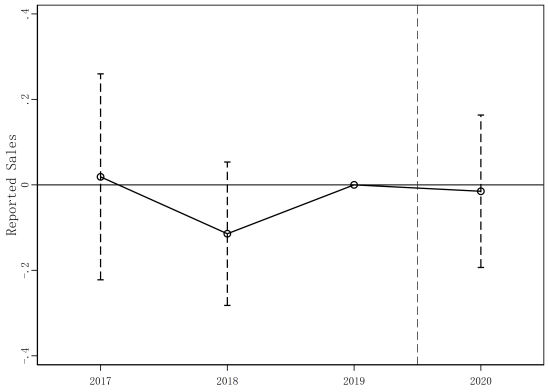
Appendix D Mechanism Analysis

Figure D1 presents the dynamic effect of the intervention on imputed sales, reported sales, and commission (i.e., the estimated β_k from equation (2) with imputed sales, reported sales, and commission as dependent variables). According to panel (a) of figure D1, the coefficients before the intervention are not significantly different from zero, while imputed sales decreased by RMB 3.724 million in 2020 and are significant at the 1% level. Panel (b) of figure D1 shows the dynamic effect on reported sales to the tax authority; the coefficients are not significantly different from zero either before or after the intervention, suggesting no significant change in reported sales. As shown in panel (c) of figure D1, the dynamic effect on commission is not significantly different from zero before 2019, whereas in 2020 the coefficient shows a significant decrease of RMB 0.186 million at the 1% level.

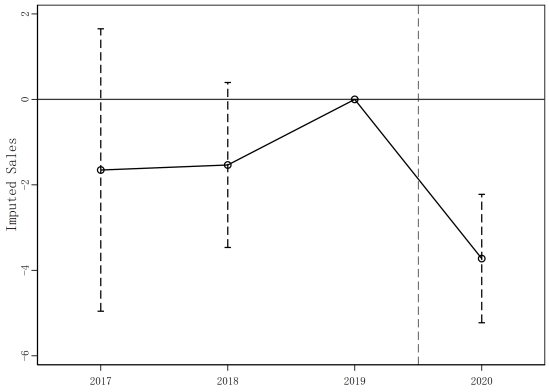
Appendix E Additional Heterogeneity Analysis

Figure E1 illustrates the dynamic difference-in-difference treatment effects segmented by firm type. Panels (a) and (c) compare MSEs with non-MSEs for sales difference and overall tax burden, respectively. Panels (b) and (d) compare general taxpayers with small-scale taxpayers for the same two outcomes. Pre-intervention estimates remain statistically indistinct from zero, supporting the assumption of a pre-treatment parallel trend.

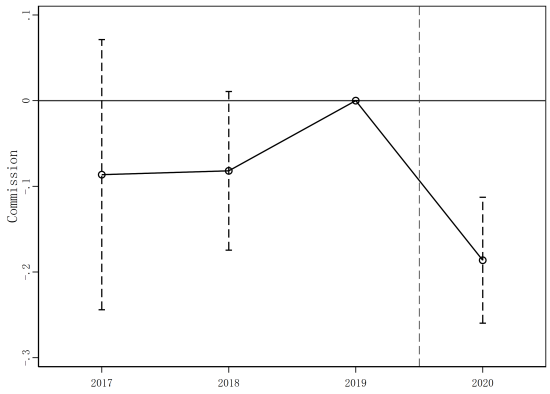
Table E1 reports the effect of the intervention on tax payments for firms in the *Suspected-intervened* group (i.e., the estimates from equation (3)). According to columns (1) and (2) of table E1, tax payments increased by around RMB 84,000 for non-MSEs after receiving the pop-up reminder, which is statistically significant at the 0.01 level and corresponds to a 78.5% increase relative to the pre-treatment mean. For MSEs, tax payments increased by RMB 6,000, which is statistically significant at the 0.1 level and corresponds to a 120% increase over the pre-treatment mean. Estimates in columns (3) and (4) of panel (a) show that general taxpayers significantly increased their tax payments by RMB 74,000, a 70.5% increase relative to the pre-treatment mean, whereas small-scale taxpayers increased their tax payments by only RMB 16,000, corresponding to a 1.143-fold increase relative to the



(a) Effect on Imputed Sales



(b) Effect on Reported Sales



(c) Effect on Commission

Figure D1: Mechanism Analysis

Notes: Panels (a), (b), and (c) present the estimated effects of pop-up reminders on imputed sales, reported sales, and commission fees for firms in the *Suspected-intervened* group, in comparison with the *Normal* and *Suspected-control* groups. The hollow markers represent the estimated effect of pop-up reminders (i.e., the coefficient β_k of the sampling years estimated from equation (2)). The dash lines are 95% confidence intervals of the estimated coefficients. The year before the intervention is omitted (i.e., the year 2019 is omitted).

pre-treatment mean; both estimates are statistically significant at the 0.01 level. Figure E2 presents the estimates from the non-parametric event study in equation (4). The estimated coefficients before 2019 are not significantly different from zero, whereas the estimates in 2020 show a clear increase.

The estimation results in table E1 and figure E2 indicate that the pop-up reminder intervention leads to statistically significant increases in tax payments for both non-MSEs and MSEs, as well as for general taxpayers and small-scale taxpayers. This suggests that the intervention effectively improves tax compliance across different segments of taxpayers, but the magnitude and relative impact vary notably by taxpayer type. Although the relative increase for MSEs is larger, the absolute increase is much smaller compared to non-MSEs. This reflects the lower baseline tax payments of MSEs, meaning that even a large proportional increase translates into a smaller absolute revenue gain. Non-MSEs, with a larger tax base, contribute a more substantial increase in tax revenue following the intervention. Again, small-scale taxpayers show a larger proportional increase but a smaller absolute increase. This may be due to differences in tax regimes, compliance costs, or reporting requirements between these groups, as well as differences in tax base size.

This finding indicates that pop-up reminders serve as an effective and cost-efficient behavioral intervention for improving tax compliance across heterogeneous taxpayer groups. Notably, the fiscal impact is more pronounced among taxpayers with larger tax bases, specifically non-MSEs and general taxpayers, underscoring the potential for targeted interventions to generate substantial revenue gains. In contrast, although the intervention yields considerable improvements in compliance among MSEs and small-scale taxpayers in relative terms, the smaller baseline levels of tax payment constrain the overall magnitude of their absolute revenue contribution.

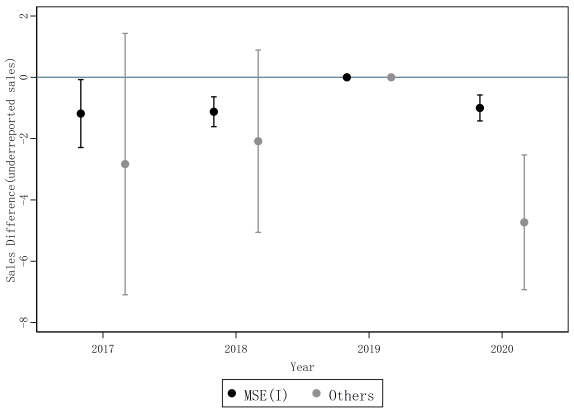
We also analyze the parametric event study estimates of the effect on tax payments, dividing firms into five groups based on their operating revenue quintiles. Figure E3 reports the effect on tax payment by operating revenue quintile. Panel (a) displays the absolute values of tax payments for each quintile of firms' operating revenue. The tax payment shows no significant change for the first quintile, a significant change of RMB 0.006 million at the 0.05 statistical level for the second quintile, a significant change of RMB 0.034 million at the

0.01 statistical level for the third quintile, a significant change of RMB 0.035 million at the 0.1 level for the fourth quintile, and a significant change of 0.216 million at the 0.05 level for the fifth quintile. The results in panel (a) suggest that the absolute increase in tax payment is driven by large enterprises. Panel (b) in figure E3 reports the parametric event study estimates on inverse hyperbolic sine transformation of tax payments by operating revenue quintile. There is no significant impact of the deterrent reminder on the percent change in tax payments for the first quintile. There is an increase of 0.592 percentage in tax payment for the second quintile, statistically significant at 0.05 level, an increase of 3.343 percent in tax payment for the third quintile, statistically significant at 0.01 level, an increase of 3.2 percent for the fourth quintile, statistically significant at 0.1 level, and an increase of 8.79 percent for the fifth quintile statistically significant at 0.05 percent level. The results in panel (b) indicate that large enterprises are primarily responsible for the relative increase in tax payments after the deterrent intervention. In general, after the intervention, the tax payments of large enterprises showed a greater increase, indicating that the rise in corporate tax payments primarily came from large enterprises.

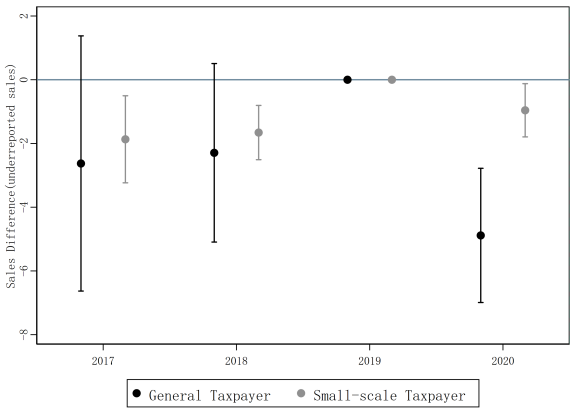
Table E2 presents the heterogeneous effect on imputed sales and commission, which provides insight into the potential channel through which the deterrent reminder affects sales difference. Panel (a) shows that the deterrent reminder significantly reduces imputed sales by RMB 3.577 million for non-MSEs and by RMB 3.604 million for general taxpayers, with both effects statistically significant at the 1% level. At the same time, the deterrent reminder has no significant effect on imputed sales for small-scale taxpayers, and it reduces imputed sales for MSEs by RMB 0.598 million , with the effect statistically significant at the 1% level. These findings suggest that the deterrent reminder mitigates underreporting behavior primarily by reducing inflated or fictitious sales, particularly among larger firms and general taxpayers. Panel (b) of table E2 presents the effect of the deterrent reminder on firms'reported sales. The estimated coefficients indicate no statistically significant impact for either MSE(I) or non-MSE(I) firms, as well as for both general and small-scale taxpayers. The estimates on the heterogeneous effects of the intervention on commission, presented in panel (c) of table E2, further support the presence of differential impacts on imputed sales across firm types. Namely, the observed decline in brushing activities appears to be largely

attributable to large enterprises, suggesting that these firms were the main contributors to the reduction in such non-compliant practices following the intervention.

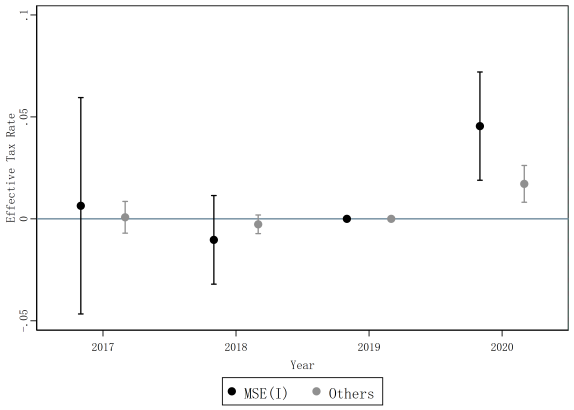
The dynamic effect on imputed sales and commission, classified by firm size, is shown in figure E4. The results in figure E4 show that large businesses'sales revenue has drastically dropped after the intervention, indicating that they have drastically curtailed brushing activities following the deterrent reminder from the tax authorities. In contrast, the imputed sales of MSEs and small-scale taxpayers remained relatively stable over time, with no statistically significant changes observed. These findings imply that the overall decrease in fictitious or inflated sales was primarily driven by large enterprises.



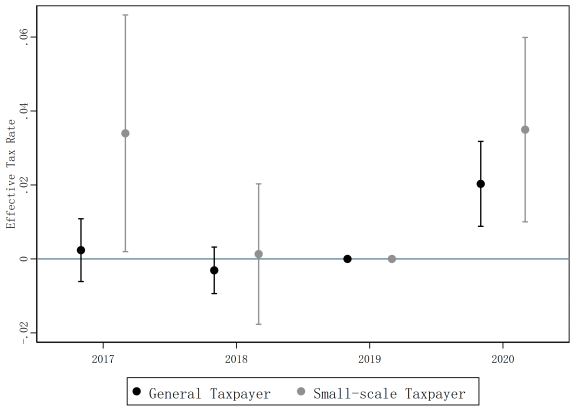
(a) Effect on Sales Difference over MSE(I)



(b) Effect on Sales Difference over General/Small-Scale



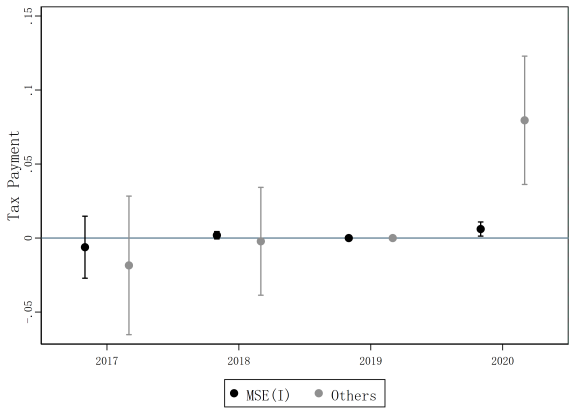
(c) Effect on Overall Tax Burden over MSE(I)



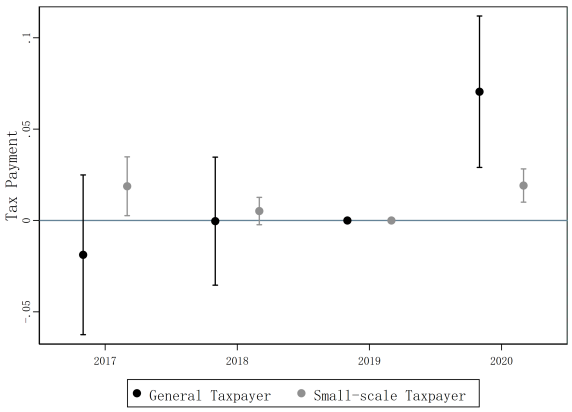
(d) Effect on Overall Tax Burden over General/Small-Scale

Figure E1: Heterogeneous Effect on Sales Difference and Overall Tax Burden by Firm Type

Notes: Panels (a) and (b) demonstrate the dynamic effect of pop-up reminders on sales difference grouped by firm type. Panels (c) and (d) present the dynamic effect of pop-up reminders on overall tax burdens relative to the control group grouped by firm type. Panels (a) and (c) compare MSE(I) firms with non-MSE(I) firms, while panels (b) and (d) compare general taxpayers with small-scale taxpayers. The hollow markers represent the estimated effect of pop-up reminders (i.e., the coefficient β_k of the sampling years estimated from equation (2)). The dash lines are 95% confidence intervals of the estimated coefficients. The year before the intervention is omitted (i.e., the year of 2019 is omitted).



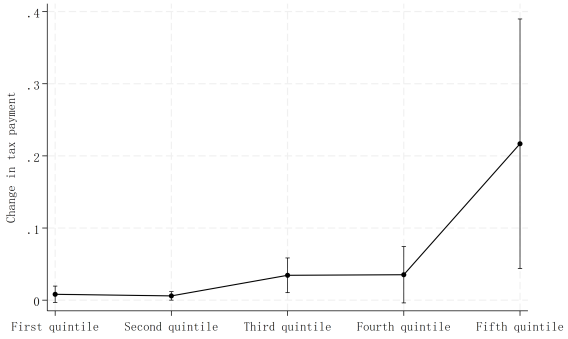
(a) Effect on tax payment over MSE(I)



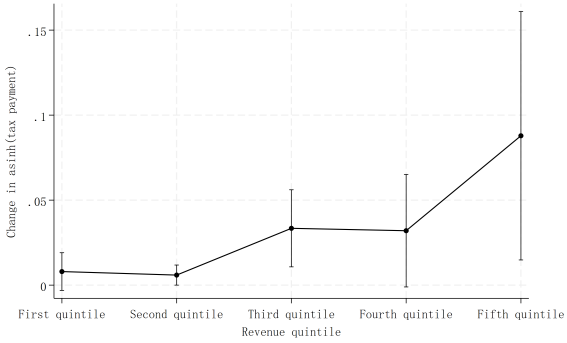
(b) Effect on tax payment over general/small-scale

Figure E2: Heterogeneous Effect on Tax Payment by Firm Type

Notes: Panels (a) and (b) demonstrate the non-parametric effect of pop-up reminders on tax payment grouped by whether the firm is an MSE(I) or not, as well as whether the firm is a general taxpayer or small-scale taxpayer. The hollow markers represent the estimated effect of pop-up reminders (i.e., the coefficient τ_k of the sampling years estimated from equation (4)). The vertical lines are 95% confidence intervals of the estimated coefficients. The year before the intervention is omitted (i.e., 2019 is omitted).



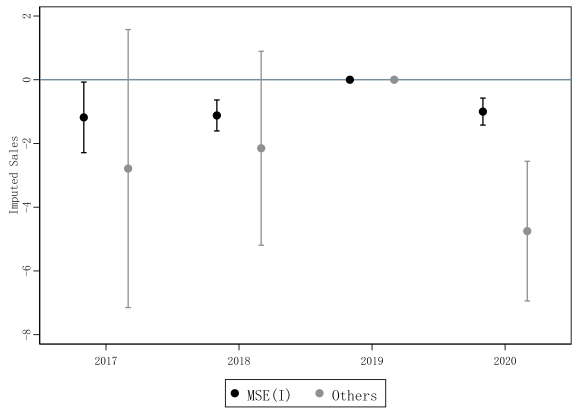
(a) Effect on tax payment over operating revenue quintile



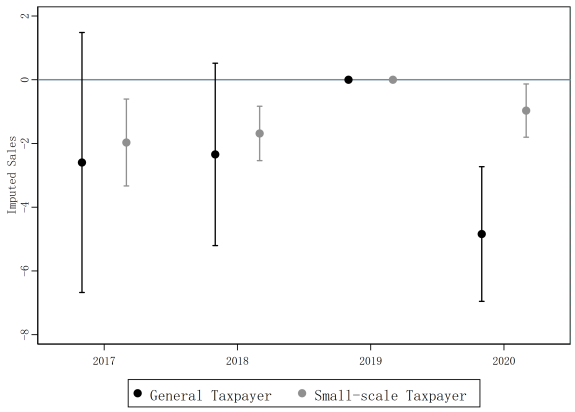
(b) Effect on asinh(tax payment) over operating revenue quintile

Figure E3: Heterogeneous Effect on Tax Payment over Operating Revenue Quintile

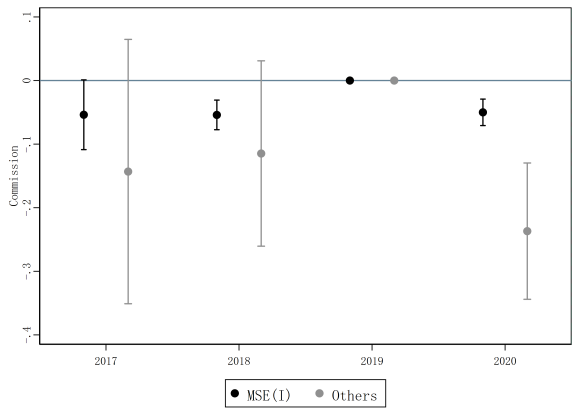
Notes: Panel (a) demonstrates the parametric event study estimates of the pop-up reminder on annual tax payment over operating revenue quintiles. Panel (b) presents the parametric event study estimates of the pop-up reminder on inverse hyperbolic sine function form of tax payment over operating revenue quintiles. The solid markers are estimated effects of pop-up reminders, and the lines represent 95% confidence intervals of the estimated coefficients.



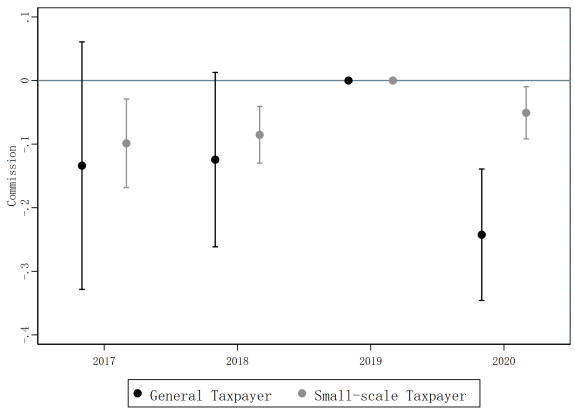
(a) Effect on Imputed Sales over MSE(I)



(b) Effect on Imputed Sales over General/Small-Scale



(c) Effect on Commission over MSE(I)



(d) Effect on Commission over General/Small-Scale

Figure E4: Heterogeneous Effect on Imputed Sales and Commission by Firm Type

Notes: This figure demonstrates the dynamic difference-in-difference estimates of the pop-up reminder on imputed sales and commission grouped by whether the firm is MSE(I) or not and whether the firm is a general taxpayer and small-scale taxpayer. The solid markers are estimated effects of pop-up reminders, and the lines represent 95% confidence intervals of the estimated coefficients.

Table E1: Heterogeneous Effect on Tax Payment

	MSE(I)	Non-MSE(I)	General taxpayer	Small-Scale taxpayer
	(1)	(2)	(3)	(4)
Post_t	0.006*	0.084***	0.074***	0.016***
	(0.003)	(0.027)	(0.025)	(0.005)
Constant	0.006***	0.102***	0.106***	0.013***
	(0.001)	(0.008)	(0.007)	(0.001)
Mean in the pre-intervention years	0.005	0.107	0.105	0.014
Observations	735	1,478	1,405	808
R^2	0.718	0.784	0.784	0.543

Notes: This table reports the event study estimates of the effect on annual tax payment and their inverse hyperbolic sine function form (asinh (tax payment)) grouped by whether the firm is MSE(I) or not and whether the firm is a general taxpayer or small-scale taxpayer.Post_t is a binary variable, whether it is in the year 2020. All regression control for firm fixed effects. Robust standard errors in parentheses are clustered at the firm level. *** p<0.01, ** p<0.05, * p<0.1.

Table E2: Heterogeneous Effect on Imputed Sales and Commission

Panel(a): Imputed sales				
Variables	MSE(I)	Non-MSE(I)	General taxpayer	Small-Scale taxpayer
	(1)	(2)	(3)	(4)
$\text{Nudge}_i \times \text{Post}_t$	-0.598***	-3.577***	-3.604***	-0.330
	(0.207)	(1.301)	(1.248)	(0.396)
Constant	1.170***	9.291***	8.988***	1.964***
	(0.011)	(0.025)	(0.021)	(0.030)
Observations	3,328	22,291	22,852	2,767
R-squared	0.939	0.787	0.786	0.807
Mean in the pre-intervention years	2.419	8.285	8.140	3.010
Panel (b): Reported sales				
Variables	MSE(I)	Non-MSE(I)	General taxpayer	Small-Scale taxpayer
	(1)	(2)	(3)	(4)
$\text{Nudge}_i \times \text{Post}_t$	-0.001	-0.008	0.054	0.005
	(0.001)	(0.142)	(0.100)	(0.004)
Constant	0.003***	2.579***	2.511***	0.036***
	(0.000)	(0.003)	(0.002)	(0.000)
Observations	3,328	22,291	22,852	2,767
R^2	0.909	0.850	0.850	0.807
Mean in the pre-intervention years	0.003	0.667	0.636	0.012
Panel (c): Commission				
Variables	MSE(I)	Non-MSE(I)	General taxpayer	Small-Scale taxpayer
	(1)	(2)	(3)	(4)
$\text{Nudge}_i \times \text{Post}_t$	-0.031***	-0.175***	-0.178***	-0.019
	(0.010)	(0.063)	(0.060)	(0.019)
Constant	0.058***	0.461***	0.446***	0.097***
	(0.001)	(0.001)	(0.001)	(0.001)
Observations	3,328	22,291	22,852	2,767
R^2	0.939	0.787	0.786	0.801
Mean in the pre-intervention years	0.117	0.411	0.404	0.145

Notes: This table reports the grouped difference-in-difference estimates of equation (1) with imputed sales, reported sales, and commission as outcome variables by whether the firm is MSE(I) or not and whether the firm is a general taxpayer or small-scale taxpayer. The definitions of Nudge_i, Post_t, MSE(I) and MSE(II) are the same as in Table 3. All regressions control for firm-level fixed effect and year fixed effect. Robust standard errors in parentheses are clustered at the firm level. *** p<0.01, ** p<0.05, * p<0.1.

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